ABSTRACT

Title of Dissertation: THE IMPACT OF SMALL-GROUP HEALTH INSURANCE REFORM

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During the 1990s, almost every state imposed restrictions on how insurers sell health insurance policies to small groups hoping to help small employers provide health benefits to workers. I use information on state small-group reforms from primary research and micro data from March Current Population Surveys to study the effect of reform on whether workers receive employer-provided health insurance in Chapter 2. Reforms preventing insurers from discriminating between healthy and unhealthy individuals should help the unhealthy, disadvantage healthy customers, and should reduce the average probability that a worker receives health insurance. I compare the changes in insurance coverage for workers in small firms before and after reform to changes in rates for workers in states that did not reform, controlling for the health insurance coverage of workers in large firms. I find that comprehensive reform decreases the probability that small employers provide health

insurance on average by almost two percentage points. Within small firms, low-risk individuals experience a 7.4 percentage point decline in the probability of receiving health insurance through their employer, while high-risk individuals appear to be unaffected.

In the third chapter, I use a unique method to combine confidential data in different locations on over 50,000 employers to study the impact of reform on the price and availability of health insurance for small employers. Regulations governing the use of the 1993 National Employer Health Insurance Survey and the 1996 Medical Expenditure Panel Survey Insurance Component prevent researchers from combining survey data for joint analysis. However, I can conduct regression analysis by constructing appropriate cross-multiplied matrices. Using an identification strategy similar to that in Chapter 2, I find that stringent reforms increase per capita premiums on average by four percent. Employers transfer over two thirds of the extra premium cost to workers by requiring higher contributions. Reform also reduces the percent of workers receiving health insurance through small firms by over two percentage points. Furthermore, the impact appears to vary by medical risk level as expected. Stringent reforms display some intended effects too; they significantly reduce the prevalence of medical underwriting practices for small employers.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Chapter 2:

CHAPTER 1

INTRODUCTION

Few topics in U.S. domestic policy stir as much debate as the issue of health care reform. Among the twenty nine industrialized countries, the United States spends by far the most resources on health care and ranks first in developing new medicine, yet continues to be last in terms of providing health insurance to its citizens (OECD, 1999).[1] In 1992, President Clinton brought the health care debate to the forefront by making universal health coverage his main domestic policy objective. But as he prepares to leave office seven years later, there are 44.3 million uninsured Americans, a number that is 7.7 million higher than at the start of his presidency.[2]

As Federal efforts to increase insurance coverage have lagged, states have intensified their efforts at reforming insurance markets. During this past decade, most states took the opportunity afforded by 'laboratory federalism' to design their own solutions to the problem of uninsurance. States have concentrated on reforming the small-group health insurance markets since about one out of every two

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[1]The US spent $4,095/capita in 1997. The closest rival, Switzerland spent only $2,611/capita. In terms of the fraction of the population with health insurance, almost all industrialized nations apart from the U.S. have a universal public health insurance system. Statistics for Turkey and Mexico indicate that they may have a lower insurance rate than the U.S., although it is not clear from the definitions used in the OECD tables. Data on investment in new medicines are taken from Phrma (1997).

[2]Figures are taken from Campbell (1999) and Foley (1993). These numbers indicate people without health insurance for the entire year.

uninsured private sector workers is attached to a business with fewer than 25 workers (Fronstin, 1998). In addition, Congress recently implemented reform at the federal level by passing the 1996 Health Insurance Portability and Accountability Act. Although it is too early to study the impact of federal small-group insurance regulation, the period from 1990 to 1996 provides fertile ground for analyzing the impact that state insurance regulations have had on access and affordability of health insurance.

The emphasis among insurers has always been to select the healthiest clients as opposed to containing the costs of the sicker ones.[3] Since clients who expect to have the highest future medical expenses will be the most eager to buy health insurance, insurers use a variety of underwriting methods, such as setting insurance premiums according to perceived health status and refusing coverage to the sick, to protect themselves against this adverse selection behavior. Insurers are particularly cautious in selecting clients in the individual and small-group insurance markets since the variance in health costs among small groups is much higher than among large groups. [4] A leading small-firm insurer claimed that during the early 1990s, "Insurability is determined by many factors, including the nature of the business..,

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[3]The sickest one percent account for 30 percent of all health spending while the healthiest 50 percent only account for three percent. Source: Berk and Monheit (1992) cited in Holahan and Nichols (1996).

[4]Large firms were also able to escape problems in private insurance markets by self-insuring their medical costs; that is, assuming all medical risks of workers and paying claims out of reserve funds.

and most importantly, the good health of all employees and dependents."[5]

Since the start of this decade, states attempted to regulate insurers and encourage risk pooling believing that increased market segmentation has contributed to the low rate of insurance provision in the small-group and individual markets. In this dissertation, I focus on state efforts to reform the small-group market by analyzing the impact of these policies on a variety of health insurance outcomes, such as the probability that a small employer offers health insurance to any of its workers, the probability that workers in small firms actually receive insurance coverage from their employers, the cost of health insurance to small employers, and the premium contributions required of workers in small firms.

The dissertation is organized as follows. In the remaining sections of this chapter, I describe the circumstances that made employers the main providers of health insurance to non-elderly Americans. I explain how this led to federal attempts to establish a system of universal coverage, and how federal inability to reach consensus shifted the responsibility onto states. In Chapter 2, I present an empirical study of the impact of small-group reform on the probability that a full-time worker in a small firm receives health insurance from his/her own employer using the March Current Population Surveys of 1992-1997. In Chapter 3, I present a study of the impact of small-group reforms on the cost of health insurance to employers and to employees, the probability that a small firm offers health insurance to its workers, the percent of workers who accept health insurance coverage and the likelihood that

[5]This quote is taken from Zellers, McLaughlin and Frick (1992), p.174.

insurance plans are subject to certain underwriting restrictions. The health insurance data used in Chapter 3 come from confidential government surveys of private employers in 1993 and 1996 which I am the first outside researcher to use. Since these data are stored at separate federal agencies, it was necessary to develop a unique method to analyze separated data subject to confidentiality restrictions. The information on health insurance reforms used in Chapters 2 and 3 come from an extensive legislative investigation I conducted using primary and secondary sources. I provide a description of this data gathering effort in Appendix A, along with details on the construction of health insurance data sets for Chapters 2 and 3. The final chapter contains concluding remarks.

1.1 Brief history of employer-provided health insurance

In 1998, 70.2 percent of all Americans were covered by private insurance, and 88 percent of these individuals received their coverage through an employer (Campbell, 1999). The first recorded instance of job-related medical insurance occurred about 200 years ago. In 1798 Congress established a compulsory hospital insurance plan for American merchant seamen (Institute of Medicine, 1993). The cost of hospital services were covered by deductions from the seamen's wages. During the next century, there were no new developments in employer provision of health care apart from a few 'sickness plans' such as the Granite Cutters Union Plan established in 1877. These plans covered very few workers, were unlike modern health insurance plans and existed only to maintain income during sickness (Starr,

1982).

A combination of historical and economic forces during the first half of this century has made employers almost the exclusive providers of health insurance to non-elderly Americans. Starting in the early 1900s some employers such as Montgomery Ward first offered a health plan to their employees. This plan was similar to those created by other companies to provide medical services as one component of 'welfare capitalism', a term that included housing, education, social assistance, and other programs intended to attach workers to their employers and discourage them from forming unions. (Institute of Medicine, 1993). Despite its early appearance, employer provision of health benefits did not spread rapidly for two reasons: companies had difficulty hiring doctors because the medical profession viewed these arrangements as a threat to the autonomy of medical practices, and workers distrusted the motives of doctors paid by their employer. The major catalyst in the growth of employer-provided health care was a ruling from the War Labor Board in 1943. It stated that the ceiling imposed on wages during World War II would not apply to employer contributions to benefits such as health insurance, forcing employers to use non-wage benefits such as health insurance to compete for workers during the war-induced labor shortage. After the war, the National Labor Relations Board ruled that employers and unions should bargain on non-wage compensation in addition to wages and work-hours. Many post-war strikes were settled with employers promising increased health insurance benefits to workers. The next boost to employer-sponsorship of health insurance came in 1954 when the

Internal Revenue Service (IRS) ruled that employers could deduct as business expenses the payments they make towards their workers' health benefits. The IRS ruling also allowed workers to omit compensation in the form of health insurance contributions made by the employer from taxable personal income. In 1958, 75 percent of the 123 million Americans with private health insurance obtained insurance coverage through employers.

In addition to the tax benefits to the firm and the workers, the purchase of health insurance through employer groups results in cost savings from risk pooling and a reduction in adverse selection. Initially, insurers community-rated all health insurance policies, that is, they treated all clients alike within broad demographic or regional groups whether they belonged to large firms or bought health insurance as individuals, and whether they were healthy or sick (Institute of Medicine, 1993, p.74). However, realizing that charging all customers the same price leads to adverse selection, insurers soon began setting prices according to health risks for small groups and individuals, a practice known as experience rating. Large firms continued to be community-rated (a practice where prices are the same for broad groups of clients) because the average health expenses among large groups do not display much variance.

Experience-rating is only one of the many factors that may be responsible for the lack of health insurance provision by small firms. In 1993, only 33 percent of firms with fewer than 10 employees offered health insurance while almost all firms (96 percent) with over 100 workers did so (NCHS 1997). Large employers enjoy

lower prices both because of the reduced variance in health costs as group size increases, and because administrative costs fall dramatically with firm size (Congressional Research Service, 1988). Morrisey et al. (1994) and surveys described in Gabel, Ginsburg and Hunt (1997) found that the reasons small employers do not offer health insurance were high cost and high year-to-year unpredictability of premiums.[6] McLaughlin (1993) and Zellers et al. (1992) report that many insurers are unwilling to sell policies to very small firms, and when a policy is offered, it is likely to exclude high-risk employees (Cantor et al., 1995). A survey of small business benefits conducted in 1992/1993 found that the majority of small-firm insurers are likely to refuse health insurance coverage for expensive illnesses if these illnesses existed prior to the start of the current insurance policy (McLaughlin and Zellers, 1994).

1.2 Federal involvement in universal health insurance

> Whoever provides medical care or pays the costs of illness stands to gain the gratitude and good will of the sick and their families. The prospect of these good-will returns to investment in health care creates a powerful motive for governments and other institutions to intervene in the economics of medicine. (Starr, 1982)

During the debate surrounding the Social Security Act of 1935, a commission

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[6] Since these studies do not control for other characteristics that may differ between small and large firms, one must bear in mind that large-firm employees may earn more than small-firm counterparts (Brown and Medoff, 1989). Leibowitz and Chernew (1992) attempt to empirically answer why small firms are reluctant to buy coverage by imputing premiums for firms not offering insurance. They find that "..it appears that the reluctance of very small firms to offer insurance is only partially explained by the high prices they face". (Leibowitz and Chernew, 1992, p.80).

was established to consider a national health insurance system. Universal health insurance did not become a part of the Social Security Act, but the US government has grappled with the idea two other times in this century.[7] At the end of World War II, President Truman re-introduced the idea of a national health insurance program financed through Social Security. He was defeated by the same group of critics who succeeded in keeping national health care out of the Social Security Act: doctors, hospitals, and private insurers. The 1960s witnessed incremental progress towards universal coverage; the government began providing medical coverage for the elderly and the poor separately through the Medicare and Medicaid programs in 1965. Our last and most memorable encounter with national health insurance was President Clinton's Health Security Act of 1994; a German-style compulsory employment-based health insurance system which incorporated managed care, private financing, choice, competition, cost management and risk-pooling. The proposal consisted of 1,364 pages and took a special White House Task Force one year to assemble. Given the complexity of the Health Security Act, it came as no surprise to many that it was defeated by Congress in the fall of 1994.

Federal attempts to solve the nation's health insurance problems have not proved successful, and the responsibility has largely been transferred to the state level. The next section describes state activity regarding the small-group health insurance market.

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[7]This discussion draws on the following: Starr (1982) and Kirkman-Liff (1997).

1.3 State small-group health insurance reform

Even before the Clinton health care proposal reached Congress, states began acting on their own to reform private health insurance. The two main reasons state efforts concentrated in the small-group market are because the large group market was thought to function effectively, and because the Employee Retirement Income Security Act (ERISA) of 1974 prevented states from regulating self-insured plans used by the majority of large employers.[8] From 1990 to 1996, almost every state enacted some combination of reform designed to prevent insurers from 'cherry picking', that is, from picking firms with low-risk workers as clients while excluding firms with high-risk workers from coverage.[9]

State legislatures recognized that encouraging more small employers to offer health insurance was a politically attractive way to decrease the number of Americans without health insurance because it did not require tax-payer money.[10]

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[8]Self insurance entails paying the health care bills of workers out of company funds, a viable alternative to commercially purchased insurance for large firms. In 1993, 80.1% of all establishments belonging to firms with over 500 workers self-insured at least one plan. For firms with fewer than 50 workers, this number was 4.5 percent (NCHS, 1997).

[9]In 1996 Congress enacted a subset of the existing state legislation through the 1996 HIPAA, effective July 1997. Once future waves of the employer surveys used here are available, I intend to examine the impact of this federal legislation as well.

[10]The following quote from the *Wall Street Journal* of June 15th, 1994 (p.A16) describes the attitude prevailing towards insurance reform:

> Its the health-care proposal everyone loves: the notion that insurance companies shouldn't be allowed to jack-up rates for sick people or others at risk of piling up big medical bills. So attractive is the concept, known as "community rating" that even many Republicans and conservative Democrats say Congress should go ahead and enact it, along with a set of other insurance-market revisions, and forget about

(continued...)

States pursued regulations of insurance companies to increase coverage since ERISA prevented them from mandating that employers provide health insurance. By 1996, 46 states enacted laws to stop what they felt was a rapid deterioration of the market for small-group insurance. A typical preamble to such laws reads as follows: "The intent of this act is to promote the availability of health insurance coverage to small employers regardless of their health status or claims experience, to prevent abusive rating practices..and to improve the efficiency and fairness of the small-group health insurance marketplace."[11] The specific small-group insurance reforms I consider in this dissertation are as follows:

- **Rating reforms** that limit insurers' ability to use health status and certain other characteristics of employees in setting premiums for small firms.
- **Guaranteed issue laws** that require insurers to accept every small group that applies. Some states guarantee only the issue of a specific low-benefit plan (often called 'bare bones plans' and treated as a separate reform in Chapter 2) that could only be sold to first-time insurance buyers. Because bare bones plans were unpopular and were only available to some small employers, I refer to laws guaranteeing the issue of more than just the bare bones plan as true guaranteed issue.

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[10](...continued)
universal coverage for now.

[11]Section 40-2209b of the Kansas statutes, added by Chapter 200, Section 1of the laws of 1992.

- **Guaranteed renewal laws** that forbid the insurer from refusing to renew a policy at the end of the year.
- **Pre-existing conditions exclusion laws** that limit the time insurers can refuse to cover illnesses present before the current policy started.
- **Portability laws** that allow a worker to move between two jobs which offer health insurance without being subject to additional pre-existing conditions exclusions periods by the new insurer. If these periods were only partially served on the previous policy, the worker can credit that time to the length he/she is required to wait on the new policy.

The common aim of these reforms is to limit insurers' ability to separate clients by health risk. Forcing insurers to pool risks among a broader group of clients should eliminate extremely high or low prices, and eliminate the practice of red-lining whereby some small employers are unable to buy health insurance regardless of the price. It is not clear, however, that these laws will improve the availability or price of insurance for the average worker in a small firm. When insurers are constrained to offer the same policy at the same price to low and high-risk clients, the terms of the common policy will be better than before reform for the high-risk individual, but worse than before reform for the low-risk client. Forcing all insurers to pool risks could induce low-risk clients to drop out of the market for insurance while encouraging more high-risk clients to enter. State-imposed small-group reform should benefit some but may make others worse off. It is not clear a priori what the

impact on the average client will be. I devote the following two chapters to empirical analyses of the impact of small-group reform to answer these questions: has the probability that a full-time worker in a small firm receives health insurance from his/her employer increased as a result of reform(Chapter 2), and has reform improved the price and availability of health insurance for small employers (Chapter 3)?

CHAPTER TWO

THE IMPACT OF SMALL-GROUP REFORM ON THE RECEIPT OF EMPLOYER SPONSORED HEALTH INSURANCE

In 1998, 44.3 million Americans were without health insurance (Campbell, 1999), a number that has risen by approximately one million annually during this decade. While an overwhelming majority of the uninsured population live in families headed by a worker, in 1997 one out of every two uninsured private sector employees was attached to a small business.[12] Since low coverage in small firms could be partly due to insurance practices such as experience-rating and redlining,[13] many states introduced laws which outlawed these practices in the small-group market. More recently the federal government has also initiated small-group reforms through the 1996 Health Insurance Portability and Accountability Act. However, to date we know little about the effects of these state and federal reforms, especially concerning adverse selection. The research reported in this chapter provides fresh evidence in one key area-whether state reforms have increased the number of small-firm workers insured through their employer.

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[12]Author calculations from Fronstin (1998). A small business is defined here and elsewhere in this chapter as one with fewer than 25 workers. To put this in perspective, workers in small businesses accounted for about 23 percent of all employment in the early 1990s. All businesses (including the self-employed) with fewer than 25 employees reporting payroll to the Internal Revenue Service (IRS) are included in this figure. These tabulations were provided by Mike Mashburn of the Census Bureau.

[13] Redlining is the practice of systematically refusing to insure groups in certain high-risk industries or occupations.

While prior studies evaluating small-group reforms have arrived at mixed conclusions,[14] many of these studies use small surveys and methods limited by the available data. Also, most studies focus on average effects while theory predicts that reforms should help certain groups and hurt others. Whether adverse selection exists in health insurance markets is a contentious issue with tremendous policy implications. Using a large, nationally representative individual level data set, I examine whether the probability that a full-time worker in a small firm receives health insurance from the employer has been influenced by state regulation of the insurance market. I also ask whether there has been heterogeneity in the effect across different risk groups[15]. I pool the March Current Population Surveys (CPS) from 1992-1997 to obtain rich demographic and employment related information for over 222,000 full-time workers. To isolate the effects of reform, I use a 'differences in differences in differences' (DDD) estimation strategy as in Gruber (1994). I compare the changes in insurance rates among workers in small firms, before and after reform. Unlike much of the previous research, I use the changes in insurance coverage among workers in large firms to form a control group. Regression results confirm that large-firm workers constitute an appropriate control group.

I expect that laws guaranteeing the issue of health insurance in combination

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[14]These studies include Jensen and Morrisey (1999), Marsteller et. al (1998), Monheit and Schone (1999), Kapur (1998), Sloan and Conover (1998), Hing and Jensen (1999), and Buchmueller and Di Nardo (1999).

[15]I use the term 'risk' to denote differences in the actuarial costs of providing health insurance as well as differences in the variance of medical costs.

with laws restricting premium variation will on average lead to a decrease in employer-provided coverage for workers in small firms due to risk pooling and adverse selection. In addition, the reforms are predicted to decrease coverage among those considered low-risk relative to high-risk individuals, presumably because of the relative changes in premiums. It is highly plausible that low-risk individuals may drop coverage faster than high-risk individuals for any given increase in premiums because of differences in the value placed on health insurance. I find empirical evidence to support these hypotheses. Specifically, the probability that a young never-married male worker in a small firm receives employer provided health insurance falls by 7.4 percentage points as a result of stringent reform, while the coverage rate for married women of childbearing age does not decrease. On average the effect is a 1.8 percentage point fall in the rate of employer coverage as a result of stringent small-group reforms. Several specification tests provide evidence that these results can be interpreted as causal.

This chapter proceeds as follows. In Sections 2.1 through 2.4 I discuss features of group insurance markets, small-group reforms, and the existing literature pertinent to this study. In Section 2.5 I describe the multivariate DDD method used in this chapter. Section 2.6 describes the surveys used, and Section 2.7 contains the basic results as well as various alternative regressions to investigate whether I can assign a causal interpretation to the observed results. In Section 2.8, I discuss the significance of my findings and suggest further steps necessary to understand how

reforms have altered the small-group insurance market.

2.1 The organization of group health insurance markets

While other industrialized nations turned to nationalized health insurance provision, the U.S. developed a system where employers provide health insurance for the vast majority of non-elderly Americans.[16] A prominent feature promoting group purchasing of health insurance is the cost-savings resulting from risk-pooling and the reduction in adverse selection. The law of large numbers leads one to expect that insurance prices should be lower in large firms compared to small firms. Large employers also enjoy sizable economies of scale in buying health insurance, as administrative costs fall dramatically with firm size (Congressional Research Service, 1988).

Several decades ago, insurers grouped all clients into relatively broad risk pools and computed premiums based on the average experience of the pool[17] (Institute of Medicine, 1993, p.74). Rothschild and Stiglitz (1976) demonstrate that

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[16]90 percent of Americans with private insurance receive coverage through their own employer or through a family member's employer in 1995 (Fronstin, 1996). Figures cited in Gruber (1998) indicate that in 1995, 71 percent of the non elderly had private coverage, while 17 percent had public health insurance (mostly through Medicaid). See Gruber (1998) for an excellent discussion of the link between health insurance and the labor market. During the last decade there has been a growth in the fraction of workers offered health benefits by their employers, but the rate of coverage for working Americans has fallen (Cooper and Schone, 1997; Gruber, 1998). Several reasons, including shifts in employment structure and increased employee contribution requirements could explain these trends (Levy, 1998; Cutler and Madrian, 1998; Farber and Levy, 1998).

[17]This practice is known as community-rating. In contrast, experience-rating refers to the practice of using information on an individual's risk factors when setting the premium.

if a group consists of individuals with different accident risks, offering one contract to all will cause the worst risk types to enter the market at any given price and lead to insurer losses. Although pooling benefitted smaller firms because of the risk averaging across a group larger than the firm itself, this system suffered from inherent instability because of risk-based adverse selection.

Insurers realized that offering one price to all caused adverse selection and they soon set the premium for each group according to its own health risks in order to lure low-risk customers away from competitors. Insurers experience-rate both to decide whether insurance should be offered at all to a small group or individual, and if so, what premium to charge. The insurer's costs of experience rating add to the already high administrative costs of selling insurance to small groups.[18] Not surprisingly, firm size is a key factor explaining whether a firm offers health insurance or not. Figures for 1993 from the National Employer Health Insurance Survey (NEHIS) show that only 33 percent of firms with fewer than 10 employees offered health insurance, while the rate was 96 percent for firms with over 99 employees (NCHS, 1997). Put another way, more than 70 percent of workers not offered health insurance through their employer were in firms with fewer than 25 workers (Long and Marquis, 1992). Empirical investigations focusing on small

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[18] Four out of five large firms with over 500 employees self-insure at least one of the plans offered to workers (National Center for Health Statistics (NCHS), 1997), leaving the commercial insurance market primarily for smaller firms. Thus any state attempt to collect revenue from commercial insurers through premium taxes etc. also makes health insurance more costly for small firms compared to their larger counterparts.

firms found several reasons, in addition to the cost reasons mentioned before, for the low rate of coverage. McLaughlin (1993) and Zellers, McLaughlin and Frick (1992) report that many insurers are unwilling to sell policies to very small firms, and even if a policy is offered, it is likely to exclude high-risk employees (Cantor et al., 1995). Morrisey, Jensen and Morlock (1994) and surveys described in Gabel, Ginsburg and Hunt (1997) found that in addition to cost, small insurers were also concerned about the variability in their premiums.[19]Contrary to expectations however, the existing literature reports that the difference between small and large-firm premiums is only one percent in 1996 (Gabel, Ginsburg and Hunt, 1997). Such a statement masks two important facts: i.e. we only observe the lower tail of the hypothetical premium distribution for small firms since a majority of this population does not buy insurance, and large-firm benefits are far more generous than those of smaller firms.[20] The combination of forces acting against small employers meant that insurance was more expensive, less readily available, and the cost more volatile than

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[19] Since these studies do not control for other characteristics that may differ between small and large firms, one must bear in mind that large-firm employees may earn more than small-firm counterparts (Brown and Medoff, 1989). Leibowitz and Chernew (1992) attempt to answer why small firms are reluctant to buy coverage by imputing premiums for firms not offering insurance. They find that "..it appears that the reluctance of very small firms to offer insurance is only partially explained by the high prices they face". (Leibowitz and Chernew, 1992, p.80).

[20]Author tabulations from the RAND/Robert Wood Johnson Foundation (RWJF) 1993 Employer Survey, for example, show that the total deductible in firms with fewer than five workers was double that of firms with over 500 workers. When I controlled for differences in benefit provision, the difference between large and small-firm single coverage premiums was close to 10 percent of a large-firm's premium. If small and large firms were equally likely to buy insurance, this difference might be much larger.

for larger groups.

2.2 State small-group health insurance reforms

As states searched for solutions to the problem of the uninsured, they turned to the small-group market because of the reasons illustrated above. Legislators felt that practices insurers used to minimize risk were unfair and tried a number of strategies to "promote the availability of health insurance coverage to small employers regardless of their health status or claims experience, to prevent abusive rating practices, ..and to improve the overall fairness and efficiency of the small-group health insurance market".[21] Between 1990 and 1996, 46 states enacted some form of small-group reform (Simon, 1999).

The types of rating reforms that states have passed in the first half of this decade are:[22]

- **Rating reforms** that limit insurers' ability to use health status and certain other characteristics of employees in setting premiums for small firms.
- **Guaranteed issue** laws that require insurers to accept every small group that applies. Some states guarantee only the issue of a specific low-benefit

[21]Section 2 of the 1992 National Association of Insurance Commissioners (NAIC) Small Employer Health Insurance Availability Model Act.

[22] See Hall (1994), Hall (1999) and Blumberg and Nichols (1998) for detailed discussions of these reforms. This list is also provided in the introductory chapter.

plan (often called **'bare bones plans'** and treated as a separate reform in my analysis) that could only be sold to first-time insurance buyers. Because bare bones plans are limited in scope, I refer to laws guaranteeing the issue of more than just the bare bones plan as true guaranteed issue.

- **Guaranteed renewal** laws that forbid the insurer from refusing to renew a policy at the end of the year.
- **Pre-existing conditions exclusion** laws that limit the time insurers can refuse to cover illnesses present before the current policy started.
- **Portability** laws that allow a worker to move between two jobs which offer health insurance without being subject to additional pre-existing condition exclusions periods by the new insurer. If these periods were only partially served on the previous policy, the worker can credit that time to the length he/she is required to wait on the new policy. Following Kapur (1998), I refer to portability and pre-existing condition limits jointly as 'portability'.

One explanation for the popularity of insurance reforms is that they are politically more attractive than actions requiring state funds. To assist state legislators, the National Association of Insurance Commissioners (NAIC) published model laws in 1991, 1992 and in 1995. Roughly 25 states followed the initial model laws in the early 1990s, making slight alterations to suit their individual needs. Of these, about eight states subsequently added stricter rating reforms. Among the rest

of the states enacting reforms, some used the NAIC model with alternate rating methods (for example, ones proposed by the Health Insurance Association of America) while others used reforms closer to community rating. Because many states followed model regulations, reforms across states and time fall into broad categories. The area in which most variation exists is rating reforms. These laws sometimes banned the use of demographic and health factors in setting premium rates, and in other instances allowed premiums to vary by certain demographic factors called 'case characteristics' while limiting variation for health reasons within and between 'rate classes'. The language of most rating statutes is not straightforward, and many allow insurance commissioners to decide on the specific restrictions upon reviewing the rates submitted by insurers. Reforms involving the rules of issue range from requiring carriers to sell just a bare bones plan to firms uninsured for the past 12 months, to requiring all products be sold on a guaranteed basis to all small firms. The three other reforms mentioned above are fairly consistent across states and time. An element of the legislation that underwent change was the size of group affected. The 1991-1992 NAIC laws defined a small group as one with 3-25 full-time employees, while later laws included employers with up to 50 employees.[23] A few states included the self-employed in the definition by making the minimum group size equal to one, but most set a minimum of two or three employees.

[23]Two states, New Hampshire and Kentucky allowed the group size to extend to 100.

Previous studies of small-group reforms have used various secondary sources to establish the timing and nature of reforms. Because of the inconsistent nature of these secondary sources, I undertook a detailed primary investigation of the laws. I gathered information from state statutes and bills, and from personal communication with 36 state insurance departments. I also compared over 25 secondary reports on these reforms to create state by state, year by year histories for 1990-1996 (details can be found in Simon, 1999, and in Appendix A). In Table 1, I list the number of states with various combinations of reforms that applied to insurance bought from 1991 to 1996. Because states enacted reforms in packages, it is not possible to assess the impact of each reform separately. Almost all states fit naturally into a three category definition of reform. Unless otherwise noted, I classify states based on whether they had A) full reform: rating reforms, issue, renewal, and portability laws, B) partial reform: includes rating reforms but excludes guaranteed issue, and C) no reform: no issue, rating or renewal laws.[24] Since barebones plans were usually sold under different terms than other health insurance and treated as distinct from the other small-group laws in the literature, I also consider them separately in the empirical section.

2.3 Predictable effects of small-group reform

The adverse selection analysis of Rothschild and Stiglitz (1976) provides a

[24]Table 1 lists every possible combination of major small-group reform enacted to show that the classification I use is appropriate.

convenient framework for anticipating how insurers may react to rating and issue laws. That model predicts that when insurers cannot tell high- and low-risk clients apart, the former will overinsure and the latter will underinsure. By determining the risk type, the insurer can avoid adverse selection by experience-rating. It can simply refuse to cover the high-risk types, or it could condition premiums on this information. Guaranteed issue prevents the insurer from refusing coverage, and rating reforms restrict the insurers ability to condition premiums on certain characteristics. Together these two reforms are likely to increase premiums for the average small-firm worker. Compared to the initial situation, guaranteed issue together with rating reforms should cause premiums to increase even further for firms with young healthy workers while premiums may fall for firms with costly workers. Portability laws act to increase the payout required in the event of the illness and thus should also increase premiums.

Predictions about the changes in employer-provided health insurance must take the behaviors of insurers, employers and employees into account. Better prices and availability may encourage high-risk firms to offer insurance, and vice versa for those experiencing price increases. Employers may pass on part of the price change to the employees in the form of higher/lower employee contributions and take-home wages. Changing conditions in the insurance market could also influence eligibility for health insurance.[25] Moving to the role played by employees, I expect their

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[25]Farber and Levy (1998) discover that increasing health costs may have led to tightening (continued...)

behavior to depend on medical risks. High-risk individuals are likely to value health insurance more than low-risk workers and thus react less to a given price change.[26] If low-risk individuals become more expensive to insure, and if employee contributions increased or wages decreased, [27] these workers may voluntarily drop coverage through the employer.[28] Taken together, the theoretical prediction is that low-risk individuals should experience decreases in coverage and high-risk individuals should experience increases in coverage.

Regarding the specific packages of reform, states with rating restrictions and guaranteed issue (coded as full reform) should experience the strongest effect, states with partial reform should feel a lesser effect, and states with minimal or no laws are expected to experience very little effect. Bare bones laws may have helped certain

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[25](...continued)
eligibility rules over time.

[26] Although studies quantifying the price sensitivity according to risk do not exist, anecdotal evidence leads one to expect that as the risks faced by a group increase, so does their need for health insurance and hence we would expect their price elasticity of demand for insurance to be lower. The literature on the price sensitivity of employers shows that employers drop coverage at the rate of about 10 percent in response to a 5 percent increase in premiums (unpublished and published work cited in Morrisey, Jensen and Morlock., 1994. Numbers are slightly higher for small firms).

[27]That certain workers who become more expensive to insure may experience wage cuts is predicted by Gruber's (1994) finding that mandated maternity benefits decreases the wages of those likely to need that service.

[28]Only about three percent of establishments required no employee contribution for single coverage, and about 11 percent required no contribution for family coverage in 1993 (author calculations from RWJ Employer data). Cooper and Schone (1997) find that offers of health insurance are on the rise while employee take-up is falling, suggesting that take-up could be more sensitive to premium increases than offers.

groups who otherwise would not buy health insurance, but they were generally considered unattractive and early evidence indicates that very few policies were actually sold (Butler, 1992).[29]

2.4 Previous studies of small-group reform

The existing literature on small-group reforms has studied the topic from different angles. Jensen and Morrisey (1999) and Hing and Jensen (1999) address whether reforms have affected employer decisions about the purchase of insurance. Monheit and Schone (1999) investigate whether offers of health insurance to workers have been affected. In this chapter, I study whether workers in small firms receive health insurance from their employer.

The earliest study to address the impact of small-group reforms from the employee's standpoint is Kapur (1999). She uses March CPS data from 1991-1997 to ask whether small-group portability and rating reforms have affected job mobility and hiring in small firms. Kapur presents convincing evidence that reforms generally improve job prospects in small firms for employees with family disabilities while worsening job prospects for older workers, and that portability reforms enacted without rating reforms may have a negative effect on individuals with poor health characteristics. These findings bode ill for the recent federal laws which enacted portability without any rating reforms.

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[29]During conversations with state insurance commission staff in various states, I learned that even in the more recent years bare bones policy sales remained weak.

Recent work by Monheit and Schone (1999), using an estimation strategy similar to the one I use in this chapter with information on about 8,000 wage-earners from the 1987 National Medical Expenditure Survey (NMES) and 6,000 from the 1996 Medical Expenditure Survey (MEPS), finds that reforms have had statistically insignificant positive effects on whether workers in small firms are offered health insurance. The authors estimate separate probits for the following four questions: what are the effects of guaranteed renewal, guaranteed issue, community rating vs. rating bands, and any reform vs. no reform from 1987 to 1996, but do not consider the effect of each type of reform conditional on others enacted at the same time.

In a recent publication, Sloan and Connover (1998) use the March CPS from 1989-1994 to study state health policies including certain types of small-group reforms. They study the impact of state policies on 1) whether a person is insured, 2) if so, whether the coverage is private, and 3) if so, whether it is from the employer. The authors create separate indicators for each reform. They find that small-group reforms exert statistically insignificant effects in all but one case: a type of community rating reform enacted by only two states had a positive effect on group coverage among people over 55 years of age, conditional on having private insurance. Marsteller et al. (1998) conduct a study of the effects of various state health reforms using CPS data for all non-elderly individuals aggregated at the state level. They discover that the combination of guaranteed issue, guaranteed renewal, portability, and limits on pre-existing exclusions has exerted a statistically significant

positive effect on a state's overall private insurance coverage rate, while the presence of any rating restriction caused a statistically significant negative effect. Buchmueller and Di Nardo (1999) also use the CPS to construct a three-state (Pennsylvania, New York and Connecticut) case study of the effects of community rating. The authors find insignificant effects of reform on insurance coverage rates.

Employer-level studies on this topic include Jensen and Morrisey (1996) and Hing and Jensen (1999). These studies, which generally use a single difference technique, find that reforms have had either a significant positive or an insignificant effect on a small firm's propensity to offer insurance.

2.5 Method

My approach compares the changes in insurance coverage for workers in small firms before and after reform to changes in coverage for workers in non-reform states, controlling for health insurance coverage of workers in large firms. The DDD method is essentially a difference between two separate 'difference in difference' (DD) estimators for small- and large-firm workers respectively. The advantage of this method is that it allows me to control for differences across states in insurance coverage trends that are not due to reforms, since reform legislation only affected small firms.

Diagram 1: DDD Chart

			Time	
			After	Before
Reform	Yes	Small	A $Y_S + \pi + \tau + \rho + \theta + \lambda + \eta$	B $Y_S + \rho + \lambda$
		Large	C $Y_M + \tau + \rho + \theta$	D $Y_M + \rho$
	No	Small	E $Y_S + \tau + \eta$	F Y_S
		Large	G $Y_M + \tau$	H Y_M

In the diagram above, expressions A through H represent coverage rates for small-firm and large-firm workers, before and after reform, in states with and without reform. Let Y_S represent the underlying propensity for a small-firm worker to receive coverage, let π represent the true impact of small-group reform on the coverage rate of small-firm workers, let τ be a year specific component common to both states and firm sizes, let ρ be a time invariant state effect that impacts both large and small firms in the reform state, let θ be a differential trend in coverage rates between the two states unrelated to reform, let λ be a component specific to small firms in states that reform, and let η be a component specific to small firms in the period after reform. One approach to judging the effect of reform is to simply difference the small-group coverage rates between the reformed and non reformed states in the second period. But this method yields a tainted estimate ($A\text{-}E=\pi+\rho+\theta+\lambda$). I can rid my estimate of one unwanted element by subtracting the difference between small-firm coverage rates in the reform and non reform states in the first period. The resulting DD estimate for small-firm workers ($[A\text{-}B]\text{-}[E\text{-}F]=\pi+\theta$) may still be confounded if the state with reform experienced any changes between the two periods relative to the no-reform state that were not due to small-group reform.

To isolate the effects of reform from these confounding influences, I need a further control group. Large-firm workers should be subject to many forces affecting that state, but should not be affected by the treatment, small-group

reforms.[30] Let Y_M represent the underlying propensity for a large-firm worker to be covered, A similar DD calculation for large-firm workers yields (C-D)-(G-H)= θ. Since the DDD estimate is the difference between the DD estimates for small and for large-firm workers, I am able to isolate the true impact of reforms on small-firm worker coverage, π. The effect of treatment calculated above can be identified through the interaction term *treatment-group (small-firm worker)*treatment-state (reform)*post-treatment (period 2)* in a regression framework.

Since the comparison above does not control for important covariates explaining whether a worker is covered through his/her employer, I embed the DDD interaction terms in a multivariate setting through the following probit specification:

$$Prob(Ins_i=1)=\Phi(\alpha+\beta X_i+\mu S_i+\omega T_t A_i+\psi A_i S_i+\delta T_t S_i+\sum_{j=1}^{3}\pi_j R_{ij} Post_{ijt} S_i)$$

Where :Φ is the standard normal distribution function, Ins_i=1 if a full-time worker i received health insurance from his/her own employer, 0 otherwise. R_j=1 if a state ever had full reform (j=1) partial reform (j=2), or bare bones laws (j=3). Each of these three indicator variables is 0 otherwise. S_i =1 if worker i is employed in a small firm (less than 25 workers), 0 otherwise. $POST_{ijt}$ =1 if reform j is effective that year in that state, 0 otherwise. The model includes state (A) by year (T) fixed effects

[30] I later test this assumption empirically and find evidence consistent with its validity.

T_tA_i[31]. π_p, the coefficient on the three level interaction term displays the effect of reform on small-firm workers.[32] The vector X includes controls for demographic and employment factors such as a worker's age, age squared, gender, marital status (five categories), interactions between gender and marital status, education (nine categories), indicators of poor health of a family member, hours worked per week, log weekly wage, number of people in the household, firm size, occupation (13 categories) and industry (13 categories).

To summarize, then, my identifying assumption in the DDD strategy is that the only factor differently affecting coverage propensity in small vs. large firms in states that implemented reform relative to states that did not reform were the reforms themselves. I discuss further issues related to this identifying assumption below.

2.6 Data

The data best suited to conduct this study are the March Current Population Surveys for the years 1992-1997. Each March, the Census Bureau interviews approximately 55,000 households consisting of roughly 150,000 individuals about employment and health insurance issues during the previous year. This large survey

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[31]This term includes all possible interactions between time and state, thus the first level terms are also included.

[32]Notice that if this had been a simpler DDD with only two years and one treatment, I would have used R*POST instead of T*A, R*S instead of S*A and S*POST instead of T*S as the second level interactions, and R*POST*S as the third level interaction.

is appealing for several reasons. First, only a few nationally representative surveys ask for health insurance, demographic and employer information. Of these, the CPS is by far the largest. Smaller surveys may not be adequate to identify effects from multiple level interactions. Second, theory predicts heterogeneity in the effects of reforms on different risk groups, and the CPS is large enough to conduct group-by-group analyses. Third, the March CPS covers the period when states enacted small-employer reform. Fourth, unlike other surveys such as the 1987 NMES and the 1996 MEPS, state identifiers are revealed in the public version of the data. Previous studies using the March CPS to answer questions about state health reforms include Kapur (1998), Sloan and Conover (1998), Marsteller et al (1998), and Buchmueller and Di Nardo (1999).

One potential shortcoming of the CPS is that certain employment questions (such as whether a person was covered by the employer's health plan) refer to any job held last year, while others (such as industry and firm size) apply to the longest job held last year. If a person's longest job last year was in a small firm that did not offer health insurance, but coverage was received from another job held that year in a large firm, I incorrectly assume that the small employer provided this worker with his/her coverage. This does not pose a problem because very few workers with health insurance (13 percent in 1996) said they held more than one job the previous year.

I select individuals from the CPS who are between 16 and 65 years of age

because younger workers might be covered through parents, and the older workers are eligible for Medicare. I include only full-time workers (whom I define as working 30+ hrs per week) because part-time workers are generally not eligible for employer provided health insurance.[33] I further restrict the sample to workers who are in the private sector and worked at least 13 weeks of the previous year. My estimation strategy uses information on individuals at large firms to control for independent forces influencing insurance markets at the same time as small-group reforms. The CPS collects firm size information in the following categories: <10, 10-24, 25-99, 100-499, 500-999 and 1000+ workers. I exclude information on workers in firms with 25-99 employees since some state laws considered them to be small-firm workers and others did not. Because the state of Hawaii mandated that employers provide health insurance to workers, I exclude respondents from Hawaii in my analysis. The final sample contains 222,166 observations. Weighted summary statistics are listed in Table 2. The weights used are the March supplemental final weights normalized within each year. Further details on data set construction are provided in Appendix A.1.

Since the March CPS of year 199x contains information on employer provided health insurance for the previous year (199x-1), the regulatory conditions as of the end of year 199x-2 are most relevant. I am able to evaluate the impact of

[33]In 1993, 53 percent of all private establishments required that employees work at least 30 hours a week to be eligible for health insurance. (Author calculations from the 1993 RWJF Employer Survey.) In addition, state statutes often use 30 hours/wk as the cutoff for defining an eligible worker.

state legislation that *went into effect* from 1990-1995 using the CPS from 1992-1997.[34] During these years many states took strong measures while several chose not to, yielding ample state and time variation to analyze. I code a state has having a certain reform if it is effective by 12/31/9x in order to apply to health insurance sold in 9x+1 and reported in the CPS of 9x+2. Because almost all states passed reform in packages and not individually, it is impossible to estimate the effects separately. While Table 1 shows the number of states implementing each combination of reform from 1992-1997, Table 3 shows the distribution of the CPS sample by reform package and year. I initially coded states with just portability laws as 'minimal reform' states, but as one can see from Table 1, very few states fit this category. I would not expect minimal reform to have much impact, and therefore I combine this category with the non reform states for the rest of the analysis.

2. 7 Results[35]

Figure 1 plots insurance coverage rates for small-firm workers as a function

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[34]An alternative interpretation of March CPS health insurance questions put forth by Swartz (1986) suggests that some respondents incorrectly answer these question for the current rather than the previous year. When I re-coded the state reform variables to follow this interpretation, my main results did not change substantially.

[35] All results presented are weighted using normalized sampling weights. However, the results are robust to the exclusion of weights. The term 'coverage' is used here as elsewhere in the paper to refer to health insurance coverage through one's own employer.

of the reform status of their state in each year.[36] The first full-reform packages were implemented by the start of 1992 (and thus should affect health insurance reported in the 1993 March CPS) in states with a higher than average coverage rate among small-firm workers. Although the nation as a whole experienced an increase in small-firm-worker coverage during the next year, full-reform states have fared much worse than both partial reform and no reform states in the years since. There seems to be signs however that coverage rates in full-reform states have increased relative to other states in the most recent year I consider. In order to separate the pattern observed in Figure 1 from events unrelated to small-group reform, I consider the trends for large-firm workers in the same groups of states over this time period. Figure 2 shows that large-firm workers in full reform states have also experienced a relative decline in coverage rates, but the magnitude of the decline is smaller than that for small-firm workers. A possible reason for this pattern is that state enactment of small-group reform was influenced by deteriorating insurance market conditions. These descriptive trends underline the need for control groups and suggest that full reform may have lowered the employer-provided insurance coverage rates for small-firm workers. In order to investigate this possibility more rigorously, I turn to results from the probit model.

The estimates in Table 4 show the probit coefficients from the multivariate DDD model, the associated marginal effects for the three variables of interest, and

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[36]The year on the X-axis refers to the year of the relevant March CPS.

standard errors for both the coefficients and the marginal effects. The control variables in the model displayed plausible results, and their coefficients and standard errors are available from the author upon request.[37] Marginal effects are calculated to simulate the difference in coverage rates for small-firm workers if they all experience reform in a certain year and if they did not. I choose 1992 as the baseline year at which to evaluate the effects of reform and I change all the appropriate year by state and year by small-firm worker interactions to reflect this fact. I calculate the predicted probability of coverage under each reform for each small-firm worker by first setting the appropriate three-level interaction to one, and then calculate the probability of coverage under no reform by setting the relevant three-level interaction(s) to zero. I average the difference between these two predicted probabilities over all small-firm workers to arrive at the marginal effect reported in Table 4.[38]

Almost all findings are consistent with the theoretical predictions and with the descriptive pattern observed in Figures 1 and 2. On average, full reform has a

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[37] In the empirical specification, the vector X in equation 1 includes the following explanatory variables, with excluded categories where appropriate to accommodate an intercept in the model: age, age^2, indicators for gender, marital status (five categories), and interactions between gender and marital status, education (nine categories), two indicators of poor health of a family member, hours worked per week, log weekly wage, number of people in the household, categorical variables for firm size, occupation (13 categories), and industry (13 categories). Removing potentially endogenous variables such as hours worked did not effect the results substantially.

[38] I re-estimate all equations as linear probability models, and I re-calculate all marginal effects alternatively at the mean of the dependent variable. Both sets of results are similar to the ones presented in Table 4.

coefficient of -0.055 with a standard error of 0.027. This translates into a statistically significant marginal effect of -1.8 percentage points on the coverage rate of small-firm workers. Given that 39 percent of full-time workers in small firms receive health insurance from their employer, this means that full reforms caused a five percent decrease on average in the rate of employer-provided coverage for full-time workers in small firms. Furthermore, the results indicate that the impact of reform was consistent with adverse selection. Coverage rates declined by a larger magnitude for workers whom insurers considered low-risk, while those considered high-risk were not significantly affected and thus fared better than the average small-firm worker. Partial reform has an insignificant effect in almost all cases.[39] Bare bones laws had no statistically significant effect, as anticipated.

I expected that high-risk workers would experience different premium changes compared to low-risk workers as a result of risk pooling, and that high-risk workers would adjust less to any given premium than low-risk workers. In support of these hypotheses, I find substantial variation in the impact of full reform among different risk populations. The first dimension along which I separate risk groups is by age, one of the most important determinants of medical expenditure and the need for health insurance. Small-group insurers generally seek out young individuals through targeting their advertising at younger populations and by specifically limiting the age of applicants for certain policies. If the elasticity of demand for

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[39]and a marginal effect that is generally smaller in magnitude than full reform.

purchasing health insurance is higher for young compared to older workers, and/or if reforms increase premiums for young relative to older workers, I expect to see a larger drop in coverage for the former. The empirical results show that small-firm workers under the age of 35 years, the median in the sample, experienced a statistically significant 4.3 percentage point decline in coverage. On the other hand, the coverage rate of older workers at small firms appears not to decline. These empirical results are consistent with the explanations put forth. Yet to the extent that states with rate setting restrictions permitted the use of age in setting rates,[40] it is hard to argue that the sample is best split along this dimension.

Ideally, one would categorize people based on specific health risks and prior medical use that insurers could detect through experience rating but are unable to use because of rating reform. Since the CPS does not contain such detailed variables, I obtained medical utilization information from the 1996 Medical Expenditure Panel Survey to investigate the link between observable demographic factors and expected health costs. These data show that married women of childbearing age (between 16 and 41 years) are more than five times as likely to be admitted to hospital, have almost three times the number of doctors visits and spend twice as many nights in hospital as never-married men under 35 years. Absent

[40]During the NAIC model legislation drafts, insurers lobbied to keep the option of setting rates based on age (Proceedings of the NAIC, 1991 cited in NAIC, 1998). Even though age and gender were restricted in the model laws that circulated in the mid 1990s, (known as NAIC Model 118) unlike in the earliest model (NAIC Model 115), upon reading the statutes of each state that adopted Model 118, I discovered that most states with rating restrictions allowed age as a rating factor, and some allowed gender. For example, of about 35 states with rating restrictions, more than half banned the use of gender (Simon, 1998).

reforms, there is evidence that insurers sometimes refused to cover businesses employing a high proportion of childbearing aged women for this reason (Zellers, McLaughlin and Frick, 1992).

When I limit my sample to young never-married men under 35 years of age, perhaps an insurers favorite demographic group, the coefficient on full reform is -0.282. This is statistically significant at p=.01 and implies a marginal effect of -7.4 percentage points. Because the average coverage rate for this population of small-firm workers is only 0.28, this represents more than a 30 percent decline in coverage. The effects of partial reform and of bare bones laws are negative but statistically insignificant. However, when I use the relatively high expenditure sample, married women of childbearing age (16-41yrs) with young children, the effect of full-reform is positive but statistically insignificant. For these same reasons, I expect that those men likely to include women of childbearing age under their policies should also be affected in a similar way. When I restricted the sample to married men under 45 yrs of age with children, I found a similar effect as for married women due to full reform in unreported results. These specifications provide further evidence that the coverage rate for low-risk workers declined due to reform, while high-risk workers were unaffected.[41] The results also show that even among young

[41]The previous footnote implies that in states allowing age and gender variation, we should not see as large a drop in coverage for young single men or as neutral an effect for young married women. When I separate states in this manner, I find that the effect for never married men under 35 years of age drops in magnitude, and that the effect for married females under 41 with children becomes a smaller positive number with a smaller t-statistic and is still (continued...)

workers, the interaction of gender, marital status and the likelihood of bearing children cause differential effects due to reform.

The next two specifications in Table 4 show that reforms affected married and unmarried workers in small firms in different ways. While married workers in small firms were unaffected by all three reforms, unmarried workers experienced a statistically significant decline in coverage of 4.2 percentage points (with a standard error of 0.013) due to full reform and 2.9 percentage points (with a standard error of 0.014) due to partial reform. These represent an eleven and an eight percent decline respectively from their base coverage rate of 0.37. Unmarried workers are more likely to drop health insurance if costs increase perhaps because they are in less need of health insurance and unlike married workers, do not bear responsibility for dependents. Another common factor used by insurers to predicts medical costs is gender. The last two rows of Table 4 show that males and females both display negative coefficients as a result of both partial and full reform, but that the effect is statistically significant only for women. [42]

Barebones laws offered a stripped-down policy covering mostly

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[41](...continued)
statistically insignificant.

[42]The coefficient on full reform for the male sample was statistically insignificant in the states that allowed some gender rating, while it was statistically significant and negative in states that banned it. For women, the reverse was true. The coefficient on full-reform was statistically significant and negative in states that allowed some gender rating, and was insignificant in states that banned it. This suggests that when insurers are allowed to use factors that predict medical expenditure, the low-risk category (in this case males) does not experience a decline in coverage.

catastrophes. The evidence from reports on barebones plans (Families USA, 1993, Butler 1992) shows that enrollment was low. There is also evidence that these policies were poorly designed and even when enacted, several states lacked insurance companies willing to market these plans (Butler, 1992). Although states hoped that bare bones plans would provide low-cost alternatives to uninsured small firms, they were unpopular and had little impact on the small-group market.

To test the stability of my findings, I undertake several specification checks. I first establish the appropriateness of my control group by asking whether small-group reforms impacted large firms. Reassuringly, I find no apparent effects of the treatment on the control group. I then change the definition of a small firm to one with fewer than 10 workers. I estimate this alternative model because smaller groups were considered higher risk pre-reform and might have been able to increase coverage (or not drop coverage as much) by being grouped together with somewhat larger firms. I find that effects of reform are smaller in magnitude and statistically insignificant for workers in smaller firms.[43] I also investigate alternative interpretations of when the effects of reform would be felt[44] and find that while the coefficient and standard error of the effects of reform diminish under certain cases, in no case does the coefficient become positive. A limitation of using the CPS is that the survey does not identify individuals with unique identifiers even though the same

[43]Full reform has a coefficient of -0.047 with a standard error of 0.033.

[44]For example, I consider the possibility that a reform would have an impact the same year that it is implemented.

individuals may be in the sample for two consecutive March surveys. In order to see whether this artificially inflates my t-statistics, I re-estimated certain models using data just on alternate years. The results indicate that the t-statistic on the effect of full reform on small firm workers increases when I consider 1992, 1994 and 1996 data, while it diminished when I consider 1993, 1995 and 1997 data. As a further specification check I investigated whether part-time (working <20 hours/week and <15hours/week separately) workers who are generally not eligible for health benefits were affected by reform. I find that as expected, the effects are smaller and insignificant among these workers.[45]

In a DDD study it is possible that the trends picked up through treatment indicators may have started many years before the treatment began. In order to test this hypothesis, I construct a data set with full-time workers from the March CPS of 1988 (the first year that the firm size question was asked) through 1991 (the last year before reforms started). I construct an F-test of whether the linear trends for small vs. large firm workers in states with vs. without reform may have been different, and I reject this possibility at the p=.05 level.

Finally, I design experiments to see what effects I would have obtained had I adopted the single level difference method used by several employer level studies that look at the effects of reform on whether a small firm offers insurance. To compare with an employer study conducted using 1993 employer information (Hing

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[45]The coefficient on full reform is 0.062 and the standard error is 0.065.

and Jensen, 1999) which found positive and statistically significant effects of full reform on the likelihood that a small employer offered insurance, I start with information on only small-firm workers in 1993 (in the March 1994 CPS). The simple comparison between states with and without reform (corresponding to A-E of Diagram 1 in Section 2 above) indicates that full reform has had statistically significant positive effects (coefficient of 0.076 and a standard error of 0.040), consistent with Hing and Jensen (1999). But when I include information on large-firm workers in 1993 as quasi-controls for underlying state differences, these effects become statistically insignificant (with a coefficient of 0.040 and standard error of 0.048). When multiple years of data are analyzed with state fixed effects, the impact become negative and statistically significant. The simulations conducted here are with employee data while the original studies use employer data. Nevertheless, they indicate the importance of controlling for underlying state differences. I have attempted to reconcile my findings with other individual level studies as well. When I limit my sample to just Pennsylvania and New York I too find insignificant effects of reforms consistent with the findings in Buchmueller and DiNardo (1999). To create a data set similar to the one used by Monheit and Schone (1999), I matched individuals across different CPS supplements of a selected years to obtain information on whether employees were offered insurance by their employers. In Appendix B of this dissertation I provide details of this analysis. I do not find any positive significant effects of reform on the probability that a small-firm worker is

offered employer-provided health insurance in these specifications.

2.8 Conclusion and Further Research

Contrary to findings in most of the existing empirical literature, the results in this chapter suggest that reforms have had statistically significant and unintentionally negative effects, at least for certain workers. These estimates are derived from a model with few untestable assumptions and a larger data set than previously used. The multivariate DDD framework compares the changes in coverage for workers in small firms before and after reform to changes in coverage for workers in non-reform states, controlling for insurance coverage of workers at large firms whom I find to be unaffected by small-group reform. Specifications checks indicate that the relationships emerging from my analysis represent more than just chance correlation.

During the 1990s, state legislators undertook comprehensive revision of insurance rules in the small-group market hoping to help small-firm employees. They acted to correct what they saw as unfair practices used by insurers to separate individuals of different medical risks. But economic theories of insurance markets warn us that preventing insurers from distinguishing between different risk groups will worsen the coverage available for healthier individuals and improve terms for those who are considered medically expensive. This chapter presents the first empirical evidence of these harms that small-group reforms may exert on employer-provided coverage among full-time workers in accordance with the theory above. I extend the existing literature by studying the effects of reform on different risk

groups. Instead of increasing coverage, or simply exerting a 'leveling effect', reforms cause coverage rates to decline with the risk-level of the group without any significant gains for high-risk individuals. The average small-firm-worker coverage rate declined by 1.8 percentage points. Relative to the average, low-risk workers fare worse while high-risk workers fare better. Theory predicts that within a voluntary insurance system, low-risk groups might leave the market or underinsure as their premiums rise while high-risk groups enter. Adverse selection of this nature may cause the price to subsequently rise even higher than the pre-reform average. The magnitude of the change for low-risk groups relative to high-risk groups is expected to be increased by their higher sensitivity to premiums if they perceive less need for health insurance.

The word 'harm' may be a poor choice since the employees losing health benefits could be receiving higher wages from the employer. Rosen's (1986) theory of compensating differentials implies that if an establishment with young male workers ceased to offer health insurance as a results of reforms, these individuals should now receive higher wages.[46]This remains an area to be explored by future research.

Several questions about effects of small-group reforms remain unanswered. I

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[46]When I regressed the log weekly wage of young single men on small group reforms (as well as the usual human capital variables and all relevant interactions for a DDD model) I find that reforms have had insignificant effects on wages. The same held true when I considered a high-risk sample, young married women with children. It is possible that laws against wage discrimination may prevent employers from adjusting wages for separate demographic groups.

find no indication that reforms have improved small-firm-worker insurance outcomes, yet this chapter does not provide empirical evidence on the mechanism through which this occurs. Presumably, one explanation is that adverse selection affects the price and thus insurance decisions at the firm and worker levels. The other is that adverse selection leads to overall price increases that demographic groups react to according to their relative valuation of health insurance. The findings in this chapter suggest that small-group health insurance reforms caused adverse selection and further research is needed to assess their full impact on the market for health insurance.

TABLE 1- NUMBER OF STATES BY REFORM PACKAGE, 1992-1997

	1992	1993	1994	1995	1996	1997
Guaranteed issue, rating reforms, guaranteed renewal, pre-existing conditions and portability (Full reform)	0	2	9	20	30	35
Rating reforms and guaranteed renewal (Partial reform)	1	11	11	8	5	2
Rating reforms, guaranteed renewal pre-existing conditions and portability (Partial reform)	0	2	3	7	6	6
Rating reforms (Partial Reform)	1	0	0	0	0	0
Rating reforms, guaranteed renewal and pre-existing conditions (Partial Reform)	0	0	0	0	0	1
Rating reforms and portability (Partial Reform)	0	2	1	0	0	0
Rating reform, guaranteed renewal, and portability (Partial ref.)	0	0	1	1	0	0
No reform	48	30	23	13	8	6
Portability	0	1	0	0	0	0
Pre existing conditions	0	1	1	1	0	0
Pre existing conditions and portability	0	0	0	0	1	0
Guaranteed issue, guarantee renewal, pre-existing conditions and portability[47]	0	1	0	0	0	0
Guaranteed renewal pre-existing conditions and portability	0	0	1	0	0	0

Source: Simon (1999a) Note: Year refers to year of the CPS-cg 1992 shows the reforms effective by 12/30/90, for insurance bought in 1991.

[47]For this case, I coded the state as full-reform since rating restrictions went into effect a few months after the other provisions.

TABLE 2- SUMMARY STATISTICS FOR FULL-TIME WORKERS, MARCH CPS 1992-1997
(Nobs=222,166)

Variable	Mean	Variable	Mean
Race		Firm Size	
Black	10.3%	Fewer than 25 workers	30.5%
Hispanic	9.0%	100-499 workers	19.6%
Other	3.4%	500-999 workers	7.4%
White	77.3%	1000+	42.5%
Education		Occupation	
1-6 th Grade	2.0%	Executive, admin. and managerial	14.7%
7-8 th Grade	1.7%	Professional specialty	11%
9 th Grade	1.6%	Technical and related support	3.9%
10 th Grade	2.5%	Sales	12.8%
11 th Grade	4.0%	Admin. support incl.. clerical	14.7%
12 th Grade	36.2%	Private household	0.4%
Some College	28.4%	Protective service	0.7%
College Graduate	17.0%	Service, except protective and household	8.9%
College +	6.7%	Farming, forestry and fishing	1.9%
Gender and Marital status		Precision prodn., craft and repair	13.2%
Divorced	11.0%	Machine operators, assemblers, inspectors	8.7%
Married	60.4%	Transport and material moving equip.	4.7%
Never married	24.6%	Handlers and laborers	4.4%
Separated	2.8%		
Widowed	1.4%	Industry	
Divorced females	6.2%	Agriculture, forestry and fisheries	1.9%
Married females	23.5%	Mining	0.8%
Never married females	9.9%	Construction	6.2%
Separated females	1.5%	Durable manufacturing	14.1%
Widowed females	1.1%	Nondurable manufacturing	10.0%
Female	42.2%	Public utilities	7.9%
Other Demographics and Job Characteristics		Wholesale trade	4.7%
Age	37.9	Retail trade	17.4%
Age^2	1567.2	Finance, insurance and real estate	8.3%
Disability in hshold	2.6%	Business and repair service	6.3%
Left job for health reasons	1.3%	Personal services-household	3.3%
Hours per week	42.8	Entertainment and recreation services	1.3%
Weeks worked last yr	48.6	Professional and related services	17.6%
Wage ($/hr)	13.40		
# persons in hshold	3.11		
92 March CPS	17.6%		
93 March CPS	17.2%		
94 March CPS	16.7%		
95 March CPS	17.1%		
96 March CPS	15.5%		
97 March CPS	15.9%		
Did worker receive health insurance from employer? (Dependent variable)			64.0%

Note: The sample consists of full time workers (defined as working a week of 30 hours or more) between the ages of 16 and 65 who worked at least 1/4 of the relevant year. Respondents from Hawaii and workers in mid sized firms (25-99 workers) have been excluded from the analysis. Weighted means are reported

TABLE 3- FREQUENCY OF REFORM BY YEAR (CPS1992-1997) ,UNWEIGHTED

(1992-1997 CPS, mid sized firms excluded. Nobs=222,166)

Reform	1992	1993	1994	1995	1996	1997
Full Reform	0	3,630	7,088	19,767	22,309	25,357
Partial Reform	744	7,963	8,744	7,346	6,242	5,495
No Reform	38,428	26,731	21,776	12,771	5,715	4,537
Bare bones plans	1,823	12,365	16,260	23,836	21,416	22,214
Total N.obs	39,172	38,324	37,138	37,879	34,266	35,389

Table 4: Probit Results
(Dependent variable=1 if worker received health insurance from employer)

			Small*Full*Post		Small*Partial*Post		Small*BBP*Post	
Sample	Obs.	Sample Mean	Probit Coef.	Marginal Effect	Probit Coef.	Marginal Effect	Probit Coef.	Marginal Effect
Whole sample	222,166	0.64	**-0.055**	**-0.018**	0.003	0.001	0.013	0.004
			[0.027]	**[0.009]**	[0.029]	[0.009]	[0.025]	[0.008]
Age<35yrs workers	92,951	0.57	**-0.145**	**-0.043**	-0.030	-0.01	0.046	0.014
			[0.042]	**[0.012]**	[-0.045]	[0.014]	[0.038]	[0.012]
Age>55yrs workers	18671	0.71	0.043	0.015	0.071	0.024	-0.096	-0.032
			[0.097]	[0.033]	[0.103]	[0.035]	[-0.090]	[0.031]
Never married males <35yrs	23,333	0.51	**-0.282**	**-0.074**	-0.022	-0.006	-0.062	-0.016
			[0.080]	**[0.021]**	[0.090]	[0.025]	[0.075]	[0.020]
Married women <41yrs with kids	20,838	0.53	0.062	0.020	-0.031	-0.006	-0.046	-0.019
			[0.096]	[0.028]	[0.098]	[0.028]	[0.089]	[0.025]
Married workers	138,416	0.66	-0.001	-0.000	0.057	0.019	0.009	0.003
			[0.034]	[0.012]	[0.036]	[0.012]	[0.032]	[0.011]
Unmarried workers	83,750	0.61	**-0.144**	**-0.042**	**-0.100**	**-0.029**	0.004	0.001
			[0.044]	**[0.013]**	**[0.047]**	**[0.014]**	[0.041]	[0.012]
Female workers	95308	0.60	**-0.072**	**-0.022**	-0.057	-0.017	-0.01	-0.003
			[0.043]	**[0.013]**	[0.045]	[0.014]	[0.039]	[0.012]
Male workers	126,860	0.66	-0.045	-0.014	0.045	0.015	0.024	0.008
			[0.036]	[0.011]	[0.038]	[0.012]	[0.033]	[0.011]

Note: Standard errors in parentheses. Marginal effects are calculated by first establishing a baseline at 1992 by changing all the variables involving time, then setting the relevant three level interaction to 1, and 0, and computing the average difference in predicted probability of the dependent variable over all small-firm workers. Standard errors for marginal effects are calculated using the delta method. Bold font indicates significance at least at the p=0.10 level.

Fig. 1 Coverage Rates for Small-Firm Workers
March CPS 1992-1997

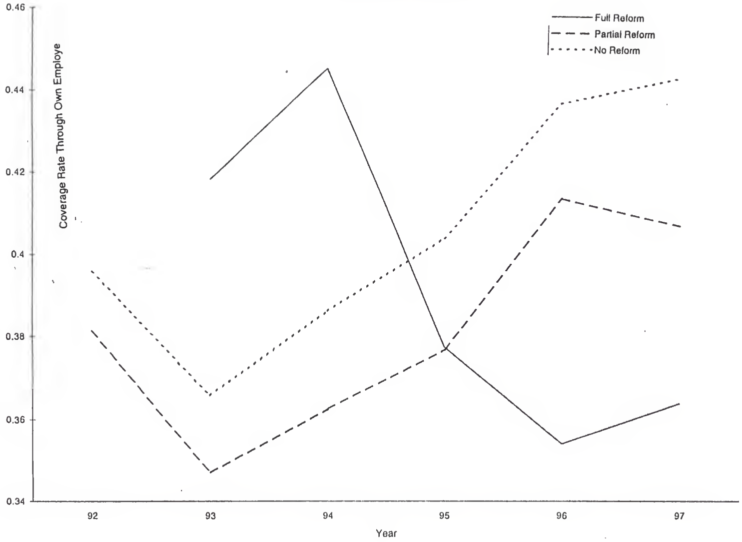
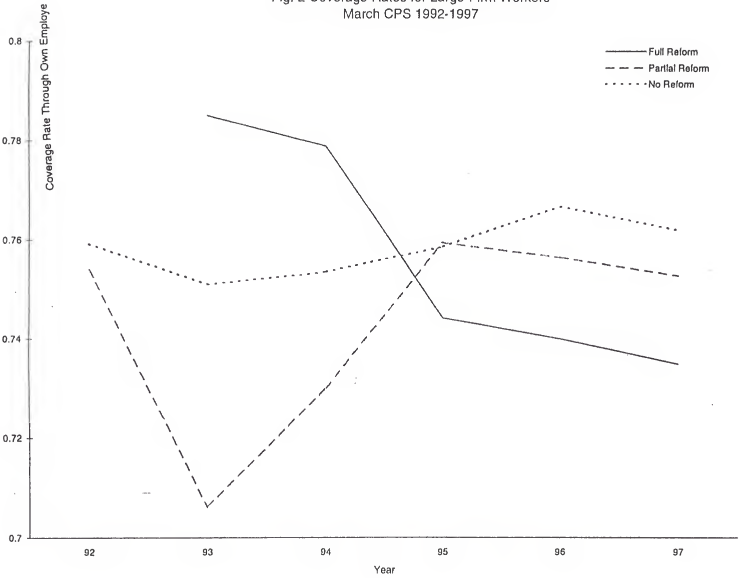


Fig. 2 Coverage Rates for Large-Firm Workers
March CPS 1992-1997



CHAPTER 3

THE IMPACT OF SMALL-GROUP HEALTH INSURANCE REFORM ON THE PRICE AND AVAILABILITY OF HEALTH BENEFITS

The fundamental changes that occurred in state small-group insurance markets during this decade provide a rare opportunity for empirical research into the impact of state restrictions on the way insurers price and sell group policies to small employers. Reforms aimed at encouraging insurers to pool risks by treating clients more equitably may reduce prices and stabilize the market for small-group insurance buyers as policy makers hope, or they may increase prices due to adverse selection. Although theory predicts that costs will rise for those firms that insurers considered low-risk, fall for high-risk firms and perhaps rise for the average firm, ultimately it is an empirical question.

In Chapter 2, I use data describing over 220,000 full-time workers from the March Current Population Surveys of 1992-1997 to investigate whether small-group health insurance reforms affect the probability that a full-time worker in a small firm receives health insurance from his/her employer. I find that stringent small-group insurance reform reduces the probability that the average worker in a small firm receives health insurance coverage by about two percentage points. Further investigation reveals that the laws affected those that may be termed low-risk workers more adversely than the average worker, but had a statistically insignificant

effect on the high-risk workers.

Despite the popularity of small-group legislation among state lawmakers, no analytical evidence exists about the impact of reform on the price of health insurance. In this chapter, I use data describing over 50,000 employers from 1993 and 1996 to test the hypothesis that small-group reform affected the price that employers pay for health insurance, the amount that employees contribute from their salary towards the cost of their health benefits, the employer's decision to offer health insurance, the percent of workers considered eligible for health insurance and the percent of workers actually receiving health insurance. I also explain whether the impact of reform on all outcomes has been different for low-risk employers than for high-risk ones, and whether reforms reduced the incidence of certain underwriting practices among small firms who offer health insurance.

To thoroughly test hypotheses concerning the impact of reform, I would ideally need at least two years of data on employers related to the provision of health insurance, and these data sets would need to be combined for the purposes of analysis. Very few large surveys of employer-provided health insurance exist, and they are generally not available to the public. The health insurance data on employers best suited to answer questions posed in this dissertation are the recently completed 1996 Medical Expenditure Panel Survey Insurance Component (MEPSIC), and the 1993 National Employer Health Insurance Survey (NEHIS). Both are protected by federal guarantees of confidentiality and are maintained at

separate locations 25 miles apart. Although I have gained access to both surveys through the submission of separate research proposals, (and in each case, I am the first outside researcher to use the survey), I cannot conduct this study using conventional regression analysis because raw data cannot be transferred from the sites. However, I am able to overcome this logistic hurdle by using a unique approach to regression analysis. Despite this limitation, I am able to compute regression coefficients using the basic properties of the matrix algebra of the OLS estimator, while still abiding by confidentiality protocols.

Theoretically, reforms preventing insurers from discerning between groups by their risk level may have distributional consequences. Premiums will likely rise for the low-risk groups, while they may fall for high-risk groups. However, the average price of health insurance may rise as insurers raise premiums to offset the increased costs imposed by adverse selection. Price changes could lead to other consequences, such as changes in the required employee contribution towards the premium, changes in the fraction of employees considered eligible for health insurance by the employer, and changes in whether employees accept health insurance. They could also influence the employer's decision to offer health insurance at all.

On the other hand, these policies might also reduce costs slightly by encouraging pooling and eliminating costly underwriting methods. Experience-rating entails administering questionnaires and investigating health histories of employees. Forcing all insurers to abstain from these practices may reduce their costs which

may lead to lower premiums. Have these reforms made health insurance more affordable? Have they led to improved coverage by small employers? Were reforms successful in reducing costly underwriting practices? The political atmosphere is once again ripe for health insurance reform and there is a great need for more research into these questions.

This chapter is a natural extension of previous work in Chapter 2. Here I examine the factors that lead to the drop in coverage that is observed using CPS data in the previous chapter. Did reforms increase the costs borne by the firms or by the workers? Did they reduce the rate at which firms offer workers health insurance benefits, or did they only lower the rate at which workers accepted this benefit? In Section 3.1 I briefly review the existing literature on the effects of health insurance reform and the adverse selection issue associated with it. Section 3.2 describes the data, including the data collection effort that recorded and codified state health insurance reforms between 1990 and 1996. Section 3.3 describes the models I estimate and a unique method of conducting regression analysis when confidentiality restrictions do not allow the outcome of interest (the price of insurance, whether insurance is offered, etc.) and the main independent variable of interest (state health care reforms) to be linked at the micro level. Sections 3.4 and 3.5 describe the results and Section 3.6 concludes.

3.1 Literature Review

The literature relevant to this chapter can be split along two dimensions. One strand of literature relates to adverse selection. The model of adverse selection most often cited in the health insurance literature is Rothschild and Stiglitz (1976). According to this framework, one would expect that when reforms prevent insurers from discriminating between high and low-risk clients, insurers will set a price that may be higher than the price offered to low-risk groups absent reforms but lower than that previously offered to high-risk groups. If these new prices cause low-risk groups to leave the market while encouraging more high-risk clients to enter, insurers may increase premiums further. It follows from this reasoning that reforms may cause a premium increase that is larger for a low-risk group than for the average firm, and a smaller increase for a high-risk group. As a result, reform may also cause a corresponding larger decline in health insurance availability for low-risk firms.

The second strand of literature relevant to this study concerns the impact of small-group reform. While many recent studies inquire about the effectiveness of state small-group regulations, almost no evidence exists on how reforms have affected health insurance premiums or employee contributions because of the lack of suitable data.[48] Several factors make the causal impact of reform on other outcomes

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[48]The only known exception is descriptive data on small employers in California between 1993 and 1995 (Buchmueller and Jensen, 1997). This study shows that average premiums decrease after reform, although the authors caution that other things such as the increased (continued...)

such as the insurance coverage rate difficult to estimate. One approach taken in the literature is to frame this as a natural experiment, but it is quite possible that the first states to reform enjoyed better health insurance outcomes at the start.[49] An additional estimation complication arises from the possibility that small-group reform occurred at the same time as other events in a state's insurance market (Buchmueller and Jensen, 1997), thus a simple comparison of outcomes in a state over time may not be adequate to isolate the impact of reform. A lack of reliable information on the timing and nature of state small-group reform has led to differences in coding in the existing literature, often making the analyses not comparable to each other.

To date, studies have looked at the impact of reform on individuals or state-averages in terms of the offers and receipts of health insurance, and the ability to move between jobs. In addition to Chapter 2, these include Buchmueller and DiNardo (1999); Sloan and Connover (1998); Kapur (1998); Monheit and Schone (1999); Marsteller et. al. (1997); and Zuckerman and Rajan (1999). In Chapter 2 I discuss these studies in more detail, and conclude that there is no consensus regarding whether reforms are beneficial or detrimental.

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[48](...continued)
penetration of managed care occurring between 1993 and 1995 could also be responsible for the decline in health insurance costs.

[49]Figures 1 and 2 in Chapter 2, regression results in Stream (1997) and a comparison of tabulations from the Employee Benefit Research Institute in Hing and Jensen (1999) suggest that states undertaking early reform may be different from those that do not in terms of outcome measures.

In contrast, employer-level studies are relatively scarce and reflect the lack of health-insurance data on employers available to researchers. Two previous studies investigate the impact of small-group reform using national employer-level data. Hing and Jensen (1999) use 1993 data on employers to compare the availability of health insurance among small firms in reform states against non-reform states. The outcomes examined are whether the firm offers health insurance, whether the plan is subject to an underwriting practice known as enrollee-exclusion,[50] and the fraction of workers who enroll in an employer's health plan. The authors find that reforms may have improved coverage, but do not increase the proportion of workers enrolled in an employer's plan and do not decrease the incidence of enrollee-exclusion. Jensen and Morrissey (1999) study effects of small-group reform (using separate indicators for guaranteed issue, guaranteed renewal, portability of coverage, pre-existing conditions limits and bare-bones plans) on a small-employer's decision to offer insurance during the first half of this decade. This study finds that overall, reform had little impact on an employer's decision to offer coverage. Most reforms display positive insignificant coefficients. The reform reducing pre-existing conditions exclusions carries a positive coefficient that is statistically significant.

Prior studies have done much to advance our understanding of small-group reform, yet there is room for improvement. The primary contribution of my study is econometric evidence of the impact of small-group reform on the cost of health

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[50]Enrollee-exclusion refers to the case when an insurer is able to prevent high-risk employees from enrolling in the health plan.

insurance, both to the employer and the employee. I also provide new evidence of the impact of reform on other outcomes such as the percent of the workforce receiving health insurance. The need for further research using data covering multiple years and more careful coding of state reforms is pointed out by Hing and Jensen (1999).

> There is a second explanation for our findings, however. Maybe states that acted earlier with full reforms were those that had better functioning small group markets to begin with. If so, the reform "effects" we reported may partly reflect these fundamental differences across markets, and we have likely overestimated the long-term benefits of reform. Unfortunately, as our data consists of firms observed all at the same point in time (1993), they cannot discern between these two possibilities. ...in classifying reform states, we relied heavily on secondary sources which may have contained errors. (P.703)

I build upon existing research by combining two large employer data sets spanning several years that allow for over-time and within-state comparisons. Because of the nature of the available data and because of the existence of multicollinearity problems, previous analyses using employer data do not account for state-level differences in the outcomes of interest, perhaps making the impact of reform harder to detect. My investigation of over two dozen secondary reports showed a number of discrepancies which I document in Simon (1999). Through a comprehensive investigation into the state legislative history of small-group reform, I am able to break the traditional reliance on secondary sources.

3.2 Data

By the late 1980s there was a general impression that practices in the insurance market for small employers had reached a stage where prices were extremely volatile and rigorous underwriting rules prevented all but the healthiest from obtaining coverage. According to a leading small-firm insurer at that time, "Insurability is determined by many factors, including the nature of the business.., and most importantly, the good health of all employees and dependents."[51]

Because many viewed the prevailing conditions in the small-group health insurance market as detrimental to all but the healthiest, states adopted five basic types of small-group regulations in the 1990s. The regulations are rating reforms, guaranteed issue, guaranteed renewal, pre-existing condition exclusion limits, and portability laws. Rating reforms restrict the insurers' ability to use health-related factors in setting premiums, while guaranteed issue and renewal provisions require insurers to sell/renew policies to all who apply. The last two regulations shorten the time during which insurers can refuse to cover illnesses existing prior to the start of a policy and increase the ease with which workers may change jobs and retain health insurance coverage. (See Hall, 1994; Blumberg and Nichols, 1998; and Simon, 1999, for further details about these reforms.) Since the most restrictive policy would be to require insurers to sell to all comers while preventing them from pricing fully according to risk, I refer to states with both guaranteed issue and rating

[51]This quote is taken from Zellers, McLaughlin and Frick (1992), p.174.

reforms as having 'full-reform', to states with rating reforms without guaranteed issue laws as having 'partial-reform', and to other states as having no reform. Although the versions of regulations adopted by various states differ somewhat, most legislators followed model laws written by organizations such as the National Association of Insurance Commissioners.

Data gathering for coding state reforms between 1990 and 1996 started in the spring of 1997. I collected over 20 secondary sources reporting on small-group health insurance reform and recorded the information provided about each state from each report (the list of publications is in the Appendix, and detailed comparisons are in Simon, 1999).These were mostly reports complied by non-profit or government entities, and often those describing early reform adoption were out of print and could only be tracked through inter-library loan. Once discrepancies became evident, I began contacting state insurance departments directly to solicit their opinions. I mailed letters to each state's health insurance regulation contact (names obtained from a list maintained by the National Association of Insurance Commissioners) asking about reforms effective in that state. I also listed the information I had gathered thus far from secondary sources about their state in these letters.[52]

After receiving responses from 36 states, I read through each 50 states' insurance codes at the Library of Congress to study the small-group insurance

[52]The Appendix contains an example of such a letter.

legislation first-hand. When only the newer versions of legislation could be found in the statutes, I referred to the actual bills to see what the effective regulations had been during the earlier years. This exercise together with the wealth of information provided by states and secondary sources helped me establish the timing and nature of reform more precisely. In certain instances, ambiguities in state legislative materials made judgement calls necessary pending responses to inquiries sent to particular state contacts. This background research resulted in a database that I intend to improve and extend to future years.

The health insurance data for this study come from two newly-available nationally-representative employer surveys that constitute a substantial improvement over existing data sources. The Medical Expenditure Panel Survey Insurance Component List Sample (MEPSIC) and the National Employer Health Insurance Survey (NEHIS) were conducted by federal agencies in 1994 and 1997 respectively to collect information about health insurance provisions and other employer characteristics during the previous year for 34,604 and 23,000 private employers.[53] These two surveys were undertaken by the federal government to produce data sets much larger than most existing private sector surveys on employer provided health insurance to produce "national and State estimates of the supply of private health insurance available to American workers and to evaluate policy issues pertaining to

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[53]The NEHIS was conducted by the National Center for Health Statistics (NCHS) in partnership with the Agency for Health Care Policy and Research (AHCPR) and the Health Care Financing Administration (HCFA), and administered by Westat. The MEPSIC was conducted by AHCPR and administered by the U.S. Census Bureau.

health insurance." (Sommers, 1999, p.iii). Large employer-level surveys have also been funded by the Robert Wood Johnson Foundation (RWJF) in 1993 and 1997 and conducted by RAND, but because of the geographical focus of the 1993 survey, they contain an overlap of only ten states between the two years.[54] The response rates for MEPSIC and NEHIS are high (71 percent for the NEHIS[55] and 70 percent for the MEPSIC) partly reflecting the confidence employers place in the federal guarantee of confidentiality. However, these confidentiality rules come at a price in terms of the scope of research that may be performed across the different data sets, and in this case they require a unique approach to regression analysis, as I shall explain below.

In order to convey the similarity between the MEPSIC and the NEHIS, I compare the two surveys' estimates of the fraction of all U.S. private-sector establishments offering health insurance, the fraction who self-insure at least one plan conditional on offering insurance, the fraction of workers who are offered health insurance, the fraction actually obtaining health insurance through the employer, and the real 1996 dollar value of premiums and employee contributions.

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[54] The RWJF Surveys contain data representative of ten states in 1993 and 12 states in 1997, although the 1997 sample was supplemented with approximately 2,000 firms from other 38 states to produce nationally representative numbers. At the present time, only the 1993 data is available to the public. The NEHIS and MEPSIC are designed to produce state-level estimates of 50 and 40 states respectively, and the MEPSIC sample was supplemented with about 1,000 observations from the remaining 10 states to produce nationally representative data. For other differences between the NEHIS and the 1993 RWJF Employer Survey, see Hing, Poe and Euller (1999).

[55] Park (1999)

Estimates from the MEPSIC and the NEHIS appear closer in some aspects than others, although I expect some differences due to the different reference periods. For example, the fraction of establishments that self-insure at least one plan has risen by about seven percentage points between the 1993 NEHIS and the 1996 MEPSIC, although the fraction of establishments offering health insurance is approximately the same. Thorpe and Florence (1999) also compare the two surveys' estimates by firm-size and industry and conclude that the two studies are very similar. Sommers (1999) notes that the NEHIS sampling design, definition of sampling strata etc. were used in the MEPSIC, although with some modification.[56] A comparison of the two survey instruments also attests to their similarity.

There are some differences between the data sets that should be noted, although I do not think they affect the analysis. One difference is the sampling frames used. While the NEHIS used the Dun and Bradstreet index of establishments to identify the universe of all firms from which to sample, MEPSIC used the Census Bureau's own Standard Statistical Establishment List (SSEL). The Dun and Bradstreet list counts all establishments that existed at any point in the year (a flow) while the SSEL counts only establishments alive at a particular point in time (a stock). Another difference is in the method of data gathering. The NEHIS relied on a telephone prescreener, the main survey was conducted through a computer

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[56]For both surveys, sampling strata were "defined by the State in which the establishment is located, the size of the establishment, and the size of the firm that owns the establishment." (Sommers, 1999, p.2) However, it appears that while the NEHIS essentially over-sampled large firms, the MEPSIC over-sampled small firms.

assisted telephone interview and questionnaires were mailed out only when respondents refused to participate over the telephone. The MEPSIC was also initiated with a telephone prescreener but the main survey was mailed out in hard copy. A telephone follow-up was conducted in both cases for respondents providing conflicting data or missing crucial fields.

3.3 Method

Although states' experiences with small-group reform span this entire decade, the focus in this paper is the 1993-1996 period during which a substantial amount of legislative activity occurred.[57] Table 1 shows the reforms which took place in various states between these two years. The three possible reform circumstances assigned to a state are 'Full Reform', 'Partial Reform' and 'No Reform' as explained above. States that do not change their reform status appear along the main diagonal and are part of the control group. While no state reversed the course of reform between these two years, three states moved from none to partial reform, 12 states moved from none to full reform and 11 states moved from partial to full reform.

If state reform occurred as a truly natural experiment and states were

[57] I consider a law to influence health insurance reported in the 1993 NEHIS if the law went into effect by 1/1/93, and to be effective for the 1996 MEPSIC if it went into effect by 1/1/96. During this period there was very little change in one aspect of reform that was included in the previous chapter, bare bones laws. There is consensus in the literature that these were weak laws.

otherwise identical, the impact on small employers could be computed by comparing the average outcomes of small-firms in reform states to those in non-reform states. If other state level differences exist and do not change over time, we could compare the difference in outcomes over time for small firms in reform states to the difference in outcomes over time in non-reform states. However, if reform states change in other ways than non-reform states, this estimation method may incorrectly attribute these changes as arising from reform. Since it seems plausible that large firms are also affected by some of these other insurance market forces but not by small-group reform itself, I use information on large firms to purge extraneous influences from the estimated impact of reform. That is, I compare the change in outcomes for small firms in reform states to the change in outcomes for large firms in reform states and then compare this to the corresponding estimate for non-reform states. The resulting 'difference-in-difference-in-difference' (DDD) estimator allows me to isolate effects due to state reform (see Gruber, 1994 for a good example of this approach, and Besley and Case, 1994 for the inadequacies of simple difference techniques.) The DDD estimate is calculated through a regression equation of the following form.

$$[1] \quad Y_i=\beta_1+\beta_2X_i+\beta_3C_i$$
$$+\beta_4S_i*F_i*POST_{it}+\beta_5S_i*P_i*POST_{it}$$
$$+\beta_6A_i \; +\beta_7T_t+\beta_8S_i+\beta_9A_i*T_t+\beta_{10}A_i*S_i+\beta_{11}T_t*S_i+\epsilon_i$$

The left hand variable is an outcome of interest such as the premium charged by

insurers to a firm i.[58] X includes establishment, firm and workforce characteristics, C represents insurance policy characteristics, F_i stands for whether the state ever implemented full reform and P_i stands for whether the state ever has partial reform, $POST_{ijt}$ indicates whether reform type j (where j=1 stands for full reform and j=2 stands for partial reform) is effective in state i in time t, A is a vector of state fixed effects, T is a vector of time fixed effects, and S is an indicator for a small firm. While most states defined a small firm as one with fewer than 25 full-time workers, some used 50 full-time workers as a cutoff.[59] Because the 25-50 worker firms are in the treatment group in some states and in the control group in others, I eliminate them from my analysis and define a small firm as one with fewer than 25 full-time workers. Respondents from Hawaii are also excluded from the sample because of that state's employer mandate to provide health insurance.

A characteristics of the two health insurance data sets used that makes this project a challenge is the strict confidentiality of responses. While the NEHIS is accessible at the National Center for Health Statistics, the MEPSIC is available only at the Census Bureau's Center for Economic Studies. Although I have access to

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[58]Other dependent variables in this study include the employee's contribution to premiums, whether the firm offers health insurance, the fraction of all workers covered, the fraction of all workers eligible, the fraction of eligible workers who accept coverage, whether an insured firm offers an HMO policy, and whether the firm experiences certain underwriting practices.

[59]According to national estimates available for 1993, about 80 percent of all workers work full time (NCHS, 1997 Table 16). A firm with 25-50 full-time workers may thus correspond to a firm with approximately 30-60 total employees assuming that the distribution of the percent of full-time workers does not vary substantially by firm size.

both surveys, they are intended to be used independently at their respective sites 25 miles apart. This restriction poses a barrier to regression analysis on the combined data, since they cannot be read simultaneously into a statistical program. Despite this limitation, I am able to compute regression coefficients using the basic properties of the matrix algebra of the OLS estimator, while still abiding by confidentiality protocols.

As an illustration, consider the simple case of regressing Y on X, where Y and X both contain data from 1993 and from 1996. Let X_{96} represent the sub-matrix consisting of 1996 data and Y_{96} the sub-vector of 1996 data.

$Y=X\beta+\epsilon$ can be written as

$$\begin{vmatrix} Y_{93} \\ Y_{96} \end{vmatrix} = \begin{vmatrix} X_{93} \\ X_{96} \end{vmatrix} \beta + \begin{vmatrix} \epsilon_{93} \\ \epsilon_{96} \end{vmatrix}$$

The solution for $\hat{\beta}$, $(X'X)^{-1}(X'Y)$ can be written using partition format as

$$\hat{\beta}=[X'_{93}X_{93}+X'_{96}X_{96}]^{-1}[X'_{93}Y_{93}+X'_{96}Y_{96}]$$

Notice that this solution does not require either of the X or Y matrices to be in their original form; the use of cross-multiplied terms such as $X'_{93}X_{93}$ helps maintain the confidentiality of survey responses.

To construct the mean square error of the regression, I make the following calculation.

$$\begin{aligned}\sum_{i=1}^{T} \hat{\epsilon}_i^2 &= (Y-X\hat{\beta})'(Y-X\hat{\beta}) \\ &= (Y'_{93}Y_{93})+(Y'_{96}Y_{96})-2\hat{\beta}'(X'_{93}Y_{93}+X'_{96}Y_{96})+\hat{\beta}'(X'_{93}X_{93}+X'_{96}X_{96})\hat{\beta}\end{aligned}$$

The regression coefficients and their standard errors can be computed by taking the cross-product matrices from the first location to the second. To compute the adjusted R^2 of the regression, I also calculate the following where Y_D refers to the de-meaned values of the dependent variable and *i* refers to a column of 1's.[60]

$$\bar{R}^2 = 1 - \frac{\hat{\epsilon}'\hat{\epsilon}/(T-K)}{Y'M^0Y/(T-1)}$$

where

$$M^0 = I - \left(\frac{1}{T}\right) ii'$$

$$\textit{Then} \quad Y'M^0Y = (Y'_{93D}Y_{93} + Y'_{96D}Y_{96})$$

In addition to computing coefficients and standard errors, I also construct F tests of restrictions on coefficients using matrix algebra.

$$F = \frac{(R\beta - q)'[\hat{\sigma}^2 R(X'X)^{-1}R']^{-1}(R\beta - q)}{J}$$

(where (Rβ-q) represents the restrictions to be tested and J represents the number of restrictions imposed) can be calculated in a straightforward manner once $(X'X)^{-1}$ and an estimate of σ^2 have already been calculated to produce coefficients and standard errors. However, it is important to note that the use of matrix algebra cannot accomplish other tasks such as corrections for heteroskedasticity unless the entire β vector can be transferred from the Census Bureau back to NCHS. [61]

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[60] This is work in progress.

[61] To see whether rating reforms lead to reduced dispersion of premiums in the small-
(continued...)

3.4. Descriptive Statistics

After imposing the sample restriction discussed above, I am left with over 50,000 private sector establishments across the two years. In Appendix A.2, I explain how the data sets were constructed and provide detailed definitions of variables. In Table 3, I provide detailed descriptive statistics of the NEHIS data, both unweighted and establishment-weighted.[62] Table 3a contains mean characteristics for small and large establishments separately, while Table 3b contains information about health insurance plans for those establishments offering health insurance by firm size. Table 3a shows that small employers are much less likely to offer health insurance, are concentrated in different industries, are less likely to have union members, have existed for fewer years, and employ less high-wage workers than large firms. About 86 percent of all employers who offered health insurance in 1993 offered only one plan (NCHS, 1997). For those employers offering more than one plan, I retain the price and plan characteristics for the plan with the most number of enrollees. Small-employers offering self-insured plans are deleted from

[61](...continued)
group market, I would also need to take the full set of coefficients from the premiums model out of CES to NCHS and compute the fitted residuals for both samples. I would then regress the squared residuals on the explanatory variables as before. Because of restrictions on removing regression output from CES, I have not conduct this part of the analysis although I am requesting permission from CES to do so.

[62] Because the MEPSIC is a new survey for Census, rules were not established about the nature of descriptive statistics that I may report at the start of this project. In fact, it now appears as though I may not be allowed to report any descriptive statistics about my sample. However, the pattern observed in the NEHIS by firm size generally holds in the MEPSIC data also.

the analysis because the Employee Retirement Income and Security Act of 1974 (ERISA) allowed these plans to avoid state regulation.[63] All premium and deductible numbers are shown in real 1996 dollars using the Bureau of Labor Statistics Consumer Price Index (CPI) for all goods.[64] The numbers in Table 3b show that plans offered by small firms in 1993 are more costly, less likely to be a form of managed care, have a higher deductible, and are less likely to cover services such as outpatient prescription drugs than are plans offered by large firms.

I estimate the various regression models first on the whole sample and then according to an approximate ranking of the medical risk-level of a firm. Because health insurance outcomes are determined to a large extent by firm size, I control for the number of employees at the firm by including linear and quadratic terms. I also include a dummy variable for whether the firm has fewer than 30 workers because that is the definition of a small firm used in this paper. If this were not included, then any effect of being an under 30-worker firm that is not captured by the linear and quadratic terms in firm size would be captured by the three-level variables that estimate the impact of reform, S*F*POST and S*P*POST in equation (1).

I expect insurance outcomes to differ by industry and I adjust for this with

[63]Because of the possibility that the decision of a small firm to self-insure may be influenced by reform, I do not include these observations in the control group.

[64]As an alternative, I used the Medical Care CPI but found that the key regression results were virtually identical. Between 1993 and 1996 the overall CPI rose by 8.5% while the medical care index rose by 14.5%

ten industry indicators.[65] A dummy variable indicating the presence of any unionized workers at the establishment allows union-negotiated benefits to differ from situations where employees have less influence. Additional employer-level control variables include the age of the firm and its square, the fraction of low-wage workers (defined as those earning below $6.50 an hour in 1996 and below $5/hr in 1993) and fraction of high-wage workers (defined as those earning above $15 an hr in 1996 and 1993).[66] Since premiums can be though of as the prices of different services multiplied by the quantity of services covered, it is important to control for the plan characteristics when considering the impact of reform on premiums and employee contributions. I use the type of plan (whether it is an HMO, conventional or mixed plan, and whether it is self-insured in the case of large firms), the amount of the total deductible, whether there is a lifetime maximum benefit and its amount, the co-payment required for outpatient treatment, and the coinsurance rate for inpatient and outpatient treatment to proxy for the quantity of coverage provided by the plan. Additionally, I include 14 indicators for specific covered services such as outpatient prescription drugs. When control variables contain missing values, I

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[65]These are: agriculture, forestry and fishing; mining, oil and gas extraction; construction; manufacturing; transport, communication and utilities; wholesale; retail; finance; business and entertainment; and professional services.

[66]In general, it is possible that wages and health insurance outcomes are endogenously determined through a wage-fringe benefit trade-off. However, the fact that wage information is captured here through a distribution of workers into three wage categories lessens this concern. When the models are re-estimated excluding wage variables, the main results do not change.

create separate indicator variables that take a value of 1 for a valid number and 0 for a missing value, and I replace missing values by zero.

3.5 Results

The sampling structure followed in both surveys produced non-nationally representative samples, thus it is necessary to use weights when computing population estimates of descriptive statistics. Both files contain an original sampling weight for each observation which is the inverse of the probability that a certain establishment was selected for the survey, as well as a final sample weight that adjusts for sample non-response. In addition, weights are adjusted to align the population estimates of these surveys with some other benchmark, a process known as 'post-stratification'. By multiplying the establishment sampling weight by the number of employees at the establishment, statistics can be computed to reflect the population of workers. By multiplying the establishment weight by the number of employees enrolled in health plans, statistics can be made to reflect the population of workers who receive health insurance from their employers. Although the unit of observation is the establishment, the theory focuses on the firm-size and perhaps the relevant weight is one reflecting the firm to which the establishment belongs. Given the choice of sampling weights available, it is unclear which weight should take precedence in this analysis.[67] From here on I concentrate on unweighted results, and

[67]It is also puzzling why they produce different estimates in some cases.

provide weighted results in separate tables below unweighted results.

The regression results presented in Tables 4 through 11 utilize the matrix technique explained above. Cross-product matrices are first computed using NEHIS data, taken to the Census Bureau and combined with similar cross products from MEPSIC. In Table 4 I first present a summary of the impact of full reform on insurance outcomes. The coefficient on S*F*POST (corresponding to the same variable in Equation 1 in the Section 3.3) shows the average impact of full reform on small firms. In Tables 5 through 10, I investigate the impact of both full reform and partial reform, on average as well as by risk groups. Thus in Tables 5 through 10 the coefficient on S*F*POST indicates the impact of full reform and S*P*POST is the impact of partial reform. The omitted category is 'no reform', so these coefficients show the effect of having partial or full reform on a small firm compared to having no reform. Since full reform is a stronger combination of restrictions on insurers than partial reform and is thus expected to have a greater impact on outcomes, Table 11 described the impact of full reform alone (including partial-reform and non-reform states in the control group). This is equivalent to estimating Equation 1 without the S*P*POST variable. Thus in Table 11 the coefficient on S*F*POST indicates the impact of full reform on small firms. The means of the dependent variables, the number of observations in a regression, and the F-statistics to test the null hypothesis that the coefficient on S*F*POST and S*P*POST are jointly equal to 0 are reported in separate columns. The critical F value at the p=0.10 level in this

case is 3. The coefficients presented in all tables are unweighted, except for Tables 5b, 6b and 7b which show the coefficients from regressions using different sampling weights.

The first question I pose is whether reforms affected the premium that a small employer pays for a single health insurance policy.[68] Ideally I would require the outcomes such as premiums and percent of employees insured to be reported at the firm level, but the available data only contain these measures at the establishment level.[69] Because almost all small firms have only one establishment, I refer to a small establishment and a small firm interchangeably in the following discussion. The results in Table 4 and Table 11 indicate that full reform increases the per-person monthly single premiums paid by small firms by about $7, although this increase is statistically insignificant Table 4. In Table 5a it appears that the increase in premiums is positive but that the magnitude and statistical significance depend on the sampling weight used. Establishment-weighted results indicate that full reform increased premiums statistically significantly by $11.63, and that the employee-weighted result shows a statistically insignificant increase of about $7. Partial reform also appears to have increased premiums statistically significantly by $10 when

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[68]For all dependent variables I use only unimputed data although I include imputed data on variables that appears only as explanatory factors. When the imputed value is missing, I create a dummy variable which takes a value of 1 if the data are valid and 0 otherwise, and include this along with the actual variable itself.

[69]Over 90 percent of all small employers in the 1993 sample belonged to single establishment firms.

considering the establishment-weighted results, but have statistically insignificant effects of between \$4.26 and -\$1.48 when looking at employee-weighted or unweighted results.[70]

Turning to employee contributions in Table 4, it appears that full reform increased the payment required of employees by \$4.9, a number that is statistically significant at the $p=0.10$ level. In Table 5a, The impact of partial reform is much smaller in magnitude and statistically insignificant. The increase in employee contributions as a result of full reform is 68 percent of the coefficient showing the increase in total premiums due to full reform (\$4.9/\$7.17) and is statistically significant when considering the unweighted results. The establishment-weighted results indicate that less than that fraction was passed on to employees (\$5.72/\$11.63), while employee-weighted numbers indicate that more than this was passed on (\$7.1/\$6.9). According to Table 11, about 65 percent (\$5.1/\$7.8) of the increase in premiums were passed on in the form of higher employee contributions, and both the numerator and the denominator are statistically significant.

An increase in the cost of health insurance may force an employer to stop offering this benefit if the employer is unable to pass this cost on to employees. Given the result above that employee contributions increase, I would not expect

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[70]Given that partial reform does not constrain insurers the way that full-reform does, it is puzzling that the impact of partial reform is as strong as it is in establishment-weighted results.

reforms to affect the employer's decision to offer coverage by much.[71] The results in Table 6 indicate that both full and partial reforms have almost no impact on whether an employer offers health insurance, although the coefficient on full reform is negative. This coefficient on full reform is -0.01 (as it in Table 11 as well) with a standard error of 0.02, and the coefficient on partial reform is 0.00 (with a standard error of 0.02). Employee and establishment-weighted results also do not indicate much impact. The second set of columns in Table 6 considers the effect of reform on the percent of workers at the establishment who are eligible to receive health insurance.[72] The coefficients indicate that both full and partial reforms have exerted negative influences on unweighted, employee-weighted and establishment-weighted results, but the standard errors are large and the t-statistics never rise above 1.6.

If premiums increase, and if employers are able to pass much of this cost on to employees, I expect that coverage and take-up rates would fall.[73] Table 7 suggests that this is a possibility. The unweighted impact of full reform on the

[71]The decision to offer health insurance is a dichotomous variable, and as such a limited dependent variable method of estimation would be most appropriate. However, matrix algebra methods are not capable of replicating maximum likelihood estimation, and a linear probability model is expected to provide a good approximation.

[72]Eligibility is defined as the number of workers at the establishment considered eligible for health insurance divided by the number of all workers at the establishment.

[73]Coverage is defined as the number of workers at the establishment who accept health insurance over the number of workers at the establishment, and take-up is defined as the number of workers at the establishment who accept health insurance over the number of workers who are eligible for it. Eligibility is usually determined by the numbers of hours worked (usually only full-time workers are eligible) and the length of time an individual has worked at the firm.

percent of workers covered by health insurance at the establishment is a statistically significant drop of 2.71 percentage points. The impact shown in Table 11 is a statistically significant decline of 2.12 percentage points[74] The impact of partial reform on the percent of workers covered in Table 7 is negative but statistically insignificant. While the employee-weighted results are weaker than the unweighted results, the establishment-weighted results are stronger. They indicate that full reform decreased the percent of workers covered by health insurance about -3.67 percentage points, while partial reform had an impact of -4.14 percentage points (these results are jointly statistically significant). Full reform had a statistically insignificant but negative effect on take-up rates (defined as the fraction of workers offered insurance who accept the offer) in both weighted and unweighted models.

The signs and magnitudes of coefficients indicate that the average small firm experienced an increase in premiums, the major share of which was passed on in the form of increased employee contributions. Also, the insurance coverage rate among workers declined although the firm's decision to offer health insurance appears unaffected. However, these results are often statistically insignificant and an equally plausible interpretation is that full and partial reforms have had little impact on the average small firm. Recall that theory predicts that full reform should have an ambiguous impact on the average small firm, but that the impact should vary by risk level.

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[74]For establishments not offering health insurance, the fraction of workers covered and eligible are set to 0.

To examine whether low and high-risk firms were differentially affected by regulations, I would ideally need information about specific health conditions of workers. Neither employer surveys I use contain such medical information. The MEPSIC collects some demographic information, but the NEHIS has virtually no details about employees that would allow me to distinguish firms by the medical risks of workers. However, a 1991 survey asked insurers to report the four-digit industry code of firms that they red-line such as florists, explosive manufacturers and hair-dressers.[75] By identifying red-lined firms in my sample based on their four-digit industry codes, I can define a high-risk group that constitutes about 15 percent of my sample.

Another less precise strategy to identify risk groups is to impute demographic characteristics for three-digit level industries using the 1990 5 percent Public Use Micro Sample (PUMS) from the Decennial Census.[76] I first identify demographic groups that are likely to differ in their use of medical services from the MEPS Household Survey of 1996. Never-married males under the age of 35 years are between two to five times less likely to see a doctor or be hospitalized than are married women of childbearing age (defined here as 41 yrs or younger) with

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[75]This list was collected as part of a survey of insurers, the results of which appear in Zellers, McLaughlin and Frick (1992). I am grateful to the authors for sharing this list with me.

[76]It is important to note that states differed in the rules by which insurers could set rates, but that about half of the states with rating reforms limited the use of at least one key demographic factor such as gender, age, etc.

children. I then use the 1990 PUMS to compute the percent of private sector full-time workers in a given state-by-three-digit-industry cell who have certain demographic characteristics, and I match this information to the establishment data sets using the same state and industry cells.[77]

This method of imputing demographic statistics is clearly a less precise method of identifying the medical risk of a firm than the previous way of selecting red-lined firms. Using the 1990 PUMS to infer the demographic characteristics of industries produces a statistically accurate estimate of the demographic characteristics in a certain state-by -industry cell only if the average demographic characteristics in industry-state cells have not changed from 1990 to 1996. Because of the changing pattern of employment from 1990 to 1996, especially among married women, I do the following exercise to get a sense of this method's accuracy. The MEPSIC does not ask for the fraction of young never-married males or the fraction of young married women with children employed by an establishment, but it does ask for the percent of female employees in the establishment's workforce. I construct an imputed value for the percent of women at the industry-state level using the PUMS. I compared this to the reported figure in the MEPSIC and found that the two measures had a correlation coefficient of

[77]I thank Kim Bayard for providing me with the cross-walk between the Census and SIC three-digit industry codes.

0.61.[78] Albeit imperfect, this method gives some indication of the relative risk level of a firm.

I next explain the method by which I classify employers belonging to certain state-industry cells as high or low-risk. In order to do this, I first observe the distribution of imputed demographics in each survey. The 75th percentile value of the imputed percent of male, never-married workers under 35 years of age (henceforth referred to as low-risk for simplicity) is 0.169 in the NEHIS and 0.179 in the MEPSIC.[79] I classify an employer as being low-risk if the imputed percent of low-risk workers in that state-by-industry cell is above 0.169 in the NEHIS or 0.179 in the MEPSIC. I identify state-by-industry cells dominated by married women employees under 41 years of age with young children (henceforth referred to as high-risk employers for a simplicity) in a similar way as being a high-risk group.

If a certain state-by-industry cell has 50 percent high-risk and 50 percent low-risk workers, the method above will classify that cell as both high-risk and low-risk. In order to avoid this possibility, I consider an alternative decision rule. I classify risk groups by using the 90th percentile values as the dividing line, and also by requiring that a low-risk group have values above the 75th percentile for the share

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[78]The mean of the imputed value is 0.34 and the mean of the reported value is x (very close).

[79]Labeling these particular groups as high and low risk is not ideal for several reasons. For example, insurers in San Francisco may consider firms with many young never-married male under 35 yrs as a high-risk firm because of the high concentration of HIV positive cases in that demographic group in that region.

of low-risk workers and values below the 25th percentile for the share of high-risk workers.

The second through fourth rows of Tables 5-7 present the results for the six different dependent variables by high and low-risk firms. In Table 5a the unweighted OLS coefficients move in the direction theory predicts (that the high-risk small firms experience premiums reductions and the low-risk small firms experience premium increases relative to the average small firm) but in general these effects are statistically insignificant. The only statistically significant impact on premiums by risk level suggests that partial reforms reduce the premium for high-risk firms by close to $26.[80] The next set of columns show that full reform has statistically significantly increased the employee contributions at low-risk firms by $16.4, while partial reform has decreased the contribution for high-risk firms by between $11.0 and $16.3. These results are all statistically significant at the p=0.1 level.

Looking at Table 6, it appears that although all coefficients are statistically insignificant, their magnitudes and signs move in directions one would expect: reform increased the proportion of high-risk employers offering health insurance and lowered the proportion of low-risk firms offering insurance. Results for the

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[80]This result is troubling because partial reforms are expected to have less impact than full reform, which displays a coefficient of -11.7 and a standard error of 9.5 here. One possibility is that insurers saw that partial reform was soon followed by full reform and acted on that expectation. Often there was a lag of over one year between the introduction and the effective dates of legislation. Thus the cutoff date I used (the start of 1993) may be too conservative, and some partial reform states could be considered full-reform states if full reform was effective anytime during 1993. When I ran alternative models using cutoff dates at the middle and the end of 1993, I found that the results did not change substantially.

regression showing the impact of reform on the percent of workers eligible for health insurance suggest the same story as for the employer's decision to offer insurance. Full reform displays a negative 3.6 percentage point impact for low-risk firms, although this is again statistically insignificant. In Table 7 the impact on the percent of workers covered by insurance is statistically insignificant, although the magnitudes and directions move once again in the direction I expect. The coefficient on full reform for red-lined small firms is 0.53 with a standard error of 3.2 while the coefficient for low-risk firms is -4.7 with a standard error of 3..[81] The take-up rates also tell a similar story.

When considering these regression results, two things must be kept in mind. First, although the coefficients appear systematically to be of the predicted sign, lack of statistical significance means that one could also conclude that reforms have had no strong effects. The second fact to bear in mind is that the impact of reform is identified from a three-level interaction term, and that the sample may need to be much larger to produce precise estimates. In Simon (1999b), the impact of reform on full-time workers was precisely calculated from a similar DDD method with over 200,000 observations on individuals, where the number of observations in the treatment category was approximately 70,000. The sample size in this paper is

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[81]To investigate the effect of reform by risk level further, I classify the risk level of a firm by requiring a low-risk firm to have more than the 75th percentile for the share of low-risk workers and fewer than the 25th percentile value for high-risk workers and vice versa for high-risk firms. Although sample sizes diminish and standard errors rise, the coefficients almost always intensify in the direction I expected: that high-risk benefit while low-risk fare worse.

50,000 at most, with approximately 13,000 observations falling into the treatment category.

In addition to the DDD estimates in Table 5-7, I also show the DD estimates for the impact of reform. Table 8 is divided into two sets of columns, one showing the results for large firms and the other for small firms separately. A good robustness check at this point would be to estimate these same equations using two other data sets of employers from 1993 and 1996 and to see whether results correspond. Although data sets of this nature are not available, a robustness test can be done with data from the public-use RWJF 1993 Employer Survey for 10 states combined with data on the same states from the MEPSIC.[82] I create a data set matching this survey to data on the same 10 states from the MEPSIC at the Census Bureau, and I estimate equations similar to (1) above using conventional means (i.e. regression procedures pre-programmed in SAS) and report the results in Table 9. Because of the limited number of states, I look at the impact of having any reform (thus full and partial reform are combined for this regression). Since these results are not comparable to those from the NEHIS/MEPSIC regressions using all states, I re-estimate the matrix regressions for the NEHIS/MEPSIC using the 10 RWJF states. In both cases I obtain negative and statistically insignificant results, but the coefficient appears larger than when all 50 MEPSIC/NEHIS states were used as

[82]The RWJF survey contains health-insurance related information for approximately 20,000 employers in New York, Minnesota, New Mexico, Washington, Vermont, Oklahoma, Oregon, Florida, North Dakota and Colorado.

seen in Table 6.

State regulators intended to promote more equitable insurance markets in addition to improving the rate of insurance coverage. While the tables above have studied the impact of reform on the latter, the effectiveness of reform should also be judged in terms of whether they reduced costly underwriting practices. In Table 10 I look at the impact of reform on whether a plan is allowed to exclude certain individuals for the life of the policy or whether the plan may impose a waiting period during which pre-existing illnesses are not covered.[83] State regulations enacted under full reform made a more concerted effort to stem these practices than regulations falling under partial reform. The results in Table 10 indicate that full reform decreased the likelihood that these practices occurred at all by a statistically significant 5 percentage points while partial reform had no impact.[84]

A new question that arises from these results is whether employers may have adjusted to a possible increase in premiums by switching to lower quality health insurance or self-insuring. While there are many ways to measure a change in quality of health insurance, I chose the type of plan offered as an indicator of quality.[85]

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[83] Disclosure considerations do not permit me to regress these two dependent variables separately.

[84] The reforms generally attempted to reduce the intensity of these practices, for example, by limiting the period during which pre-existing conditions are excluded from a policy. Although it would be desirable to see the impact of reform on the intensity of practices as well, this information is not available for both surveys.

[85] In future work I plan to look at whether a certain benefit like prescription drugs is offered (continued...)

HMOs are often thought of as a cheaper alternative to conventional plans. In an unreported table, I estimate a DDD model where the explanatory variables are the same as in equation (1) but the dependent variable is whether the firm offers a managed care product or not.[86] It appears that small firms may indeed be more likely to offer a managed care plan in states that reform, a suggestion that has been advanced elsewhere (Buchmueller and Di Nardo 1999, and Buchmueller and Jensen 1998). Reforms are correlated with the probability that a small firm offers a managed care product by almost 5 percentage points. However, when I compute the DD estimate for large firms (also unreported in this draft) this impact is also positive and statistically significant suggesting that managed care penetration may be occurring at the same time as reform and that small employers are drawn to these plans more than large firms, but that this may not be caused by reform. Looking at whether a firm self-insures any plan (in unreported tables),[87] I find that the DD estimate for small firms suggest the correlation is positive and statistically significant, although the DD estimate for large firms is negative and statistically significant. No conclusion can be drawn from this analysis about whether this is an effect of reform or not, but it is an issue worth probing further.

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[85](...continued)
as a measure of generosity.

[86]I look at whether the employer offers a managed care product at all, not just whether it is the most popular plan that is offered.

[87]For this analysis, I include small firms that self-insure their health plans.

In summary the results appear to indicate that full reform caused premiums to rise for the average small firm. Although theory does not make direct predictions on the movement of employee-contributions, the fact that they also increase by a similar magnitude provided an internal consistency check of whether the results obtained are plausible, and is also consistent with the notion that employees ultimately bear the cost of fringe benefits. The percent actually receiving health insurance coverage decreases by more than two percentage points. It appears that the small employer's decision to offer health insurance was not affected by much as a result of reform, and that the percent of workers eligible for health insurance displays statistically insignificant negative effects .

Results for separate risk groups also appear to support the hypothesis that full-reform increased premiums and reduced insurance coverage rates for low-risk small employers while having the opposite impact on high-risk employers. However, not all results are as expected (for example, it is troubling that partial reform, and not full reform has caused bigger changes in premiums by risk groups in some cases), and are often statistically insignificant. Nevertheless, the general sign and magnitude of coefficients indicate that these results are consistent with adverse selection behavior.

3.6 Conclusion

This chapter uses a large data set of 50,000 employers spanning all states over a time period of three years as well as primary legislative data on state reforms to investigate impact of state regulation in the health insurance market for small employers. These insurance reforms aimed to improve conditions for small employers by encouraging insurers to pool the risks of their clients. While state policy makers hope that these regulations would help small employers, I hypothesize that reforms could lead to losses for the low-risk population, gains for the high-risk population and perhaps a decline in insurance outcomes for the average small employer because of adverse selection behavior.

I use a unique method of arriving at OLS estimates with confidential data sets using matrix algebra, and an identification approach which compares the difference in health insurance outcomes among small firms in states that reformed before and after reform while controlling for several other factors that could incorrectly be attributed to reform. I find that full reform appears to increase premiums and employee contributions for the average small firm by about $7 unweighted regressions (and more in weighted results), decrease the percent of workers covered by the average small employer's health insurance policy by more than 2 percentage points. Given that employers are able to adjust to the impact of reform on premiums by increasing employee contributions or tightening eligibility conditions for health insurance, it is not surprising that the decision to offer health

insurance does not appear to be affected substantially by reform. The good news, however, is that stringent regulations have succeeded in reducing the incidence of costly underwriting practices.

As in Chapter 2, this study also finds evidence to suggest that low-risk groups were made worse off by reforms while high-risk groups were not harmed. The premiums, employee contributions, the percent of workers covered by health insurance and the employer's decision to offer health insurance all display signs and sometimes statistically significant coefficients that are consistent with this theory. Moreover, the impact of small-group reform measured in this paper are consistent with results in Chapter 2. The impact of full reform on the probability that the average small-firm worker received insurance from his/her employer in Chapter 2 was a statistically significant drop of close to two percentage points. Here, the impact of full reform on the percent of workers given health insurance by small firms is a statistically significant drop of 2.7 percentage points in unweighted results. The impact of full reform on low-risk individuals at a small firm in Chapter 2 was a statistically significant drop of seven percentage points. In this study, the impact of full-reform on the percent of workers given health insurance by a small low risk employer is a statistically insignificant drop of five percentage points.

Although the results appear plausible and robust in many ways, some questions warrant further investigation. One is that results appear sensitive to the choice of sampling weights used regressions. I concentrate on unweighted results

and present weighted results for comparison. The other is that partial reform, which should have a weak impact since insurers are still allowed to deny health insurance to clients under this regime, appears to sometimes influence outcomes to an equal or greater degree than full reform. The third is that most results for different risk groups are statistically insignificant and one could also interpret them to indicate that small-group reforms have had little impact on the healthy vs the sick.

In conclusion, the contributions of this chapter are threefold. First, I present new evidence on the impact of small-group reform on small employers. Prior to this study, virtually nothing was known about what small-group reforms have done to the price of health insurance . Second, I gathered reliable data on state reform though direct means; through contact with state officials and from state legislative records and was able to avoid inconsistencies apparent in secondary sources. Third, I use a unique method to analyze confidential data without which no econometric comparisons could be made between the MEPSIC and the NEHIS. Surveys administered under strict guarantees of confidentiality are invaluable for researchers, yet new techniques such as the one developed in this study are crucial if these data are to be used in an effective manner.

Table 1:Small-Group Reform Changes, 1993-1996

1993⇓ 1996⇒	Full Reform	Partial Reform	No Reform
Full Reform	CT IA KS MA NC OH RI VT WY ID MT		
Partial Reform	DE FL LA ME ND NE NM OK SC SD	AR GA IN OR WI WV	
No Reform	AK CA CO KY MD MN MO NJ NY TX VA WA TN NH	IL MS UT	AL AZ DC MI NV PA

Table 2: NEHIS and MEPSIC Comparisons

Variable	MEPSIC (1996)	NEHIS (1993)
Percent offering major health insurance	53.2%	51.6%
Percent of insured who self-insure at least one plan	28.4%	21%
Percent of workers offered	81.5%	81.7%
Percent of workers covered	85.4%	84.8%
Premium for single coverage	$166.4	$163#

Sources: AHCPR (1999) and NCHS (1997)

#NOTE: This figure is preliminary and should not be quoted. Source: NCHS.

Table 3a Descriptive Statistics for 1993, All Establishments

	Small Firms			Large Firms		
Variable	N	Unweighted	Weighted	N	Unweighted	Weighted
Fraction offering	16608	0.42	0.40	15125	0.96	0.95
Firm size (# employees)	16608	7.75	6.75	15125	18147	12052
Agriculture, forestry and fishing	16608	0.04	0.04	15125	0.004	0.002
Mining, extraction	16608	0.10	0.10	15125	0.031	0.03
Construction and manufacturing	16608	0.07	0.07	15125	0.19	0.10
Transport, communication and utilities	16608	0.035	0.033	15125	0.08	0.07
Wholesale	16608	0.07	0.076	15125	0.07	0.095
Retail	16608	0.23	0.22	15125	0.24	0.28
Finance	16608	0.07	0.07	15125	0.10	0.15
Business and entertainment	16608	0.16	0.16	15125	0.08	0.09
Professional services	16608	0.21	0.21	15125	0.18	0.16
Union presence	15924	0.02	0.02	13123	0.13	0.08
Age of firm (yrs)	15818	20.4	19.2	14481	50.6	48
% low wage	15090	16.4	16.3	10891	9.9	12.1
% high wage	15014	17.4	18.84	10430	22.2	21

Table 3b Descriptive statistics for 1993, Establishments Offering Insurance

	Small Firms			Large Firms		
Variable	N	Unweighted	Weighted	N	Unweighted	Weighted
Premiums	5791	190.13	195.4	10774	181.34	182.4
Self insured	-	-	-	10774	0.49	0.45
HMO	5791	0.12	0.14	10774	0.16	0.19
Mixed type	5791	0.35	0.36	10774	0.40	0.39
Total deductible	5157	335	336	10392	208	208
Maximum benefit	3897	4878100	4935922	9513	3973760	4374483
Copay outpatient	3369	4.6	4.76	5331	6.5	6.5
Coinsurnace,inpatient	5622	15.9	15.3	10417	14.2	14
Coinsurance,outpatient	4119	11.5	11.12	7126	14.6	14.5
Routine mammograms	4888	0.77	0.77	10262	0.78	0.78
Routine physicals	5302	0.63	0.63	10493	0.58	0.58
Routine pap-smears	5002	0.75	0.75	10353	0.76	0.75
Child immunizations	4374	0.66	0.67	10263	0.69	0.67
Well-baby care	4356	0.69	0.69	10299	0.69	0.67
Well-child care	4286	0.67	0.67	10160	0.63	0.63
Outpatient drugs	5485	0.77	0.76	10579	0.94	0.93
Routine dental care	5585	0.21	0.20	10587	0.36	0.34
Orthodontics	1091	0.43	0.44	3676	0.59	0.54
Nursing home care	4082	0.32	0.34	9461	0.51	0.52
Home health care	4211	0.55	0.54	9728	0.84	0.83
Inpatient mental health	4710	0.84	0.83	10328	0.96	0.95
Outpatient mental health	4319	0.73	0.71	10181	0.94	0.93
Alcohol/drug abuse care	4482	0.75	0.74	10227	0.93	0.92

Table 4. The Impact of Full Reform , Summary OLS Results [88]

	N	Mean	S*F*Post
Premiums	26,651	181.1	7.2
			(4.8)
Employee Contribution	28,052	32	**4.9**
			(2.8)
Decision to offer	50,485	0.66	-0.01
			(0.02)
Coverage rate	47,598	42.9	**-2.7**
			(1.5)
Medical Underwriting	33,094	0.58	**-0.05**
			(0.03)

[88]Bold font indicates statistical significance at the p=0.10 level. Standard errors are in parentheses.

Table 5a: OLS Results, Single Premiums and Employee Contributions.

	Premium					Employee Contribution				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	26,651	181.1	7.17 (4.83)	-1.48 (5.16)	2.1	28,052	32	**4.9** **(2.8)**	-0.59 (3.0)	2.6
Red-lined industries	4,372	189.4	-2.19 (12.1)	-9.84 (13.3)	0.6	4,543	37	-7.86 (7.4)	**-16.3** **(8.08)**	2
Above 75th percentile for % never married males under 35	5,503	178.9	7.04 (10.83)	-0.43 (12.5)	0.2	5,703	37.1	**16.4** **(6.5)**	-3.0 (7.5)	**4.6**
Above 75th percentile for % young married women with kids	6,821	183.1	-8.46 (9.35)	**-26.0** **(9.2)**	**4.0**	7,229	34.7	-5.9 (5.87)	**-11.0** **(5.78)**	1.8

Table 5b: Weighted Results, Single Premiums and Employee Contributions

	Premium					Employee Contribution				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	26,651	187.7	**11.63** **(5.23)**	**10.0** **(6.1)**	3.0	28,052	29.9	**5.72** **(2.9)**	1.35 (3.5)	1.9
Employee weighted results	26,651	180.8	6.9 (5.43)	4.26 (6.5)	0.7	28,052	31.2	**7.1** **(3.1)**	2.5 (3.7)	2.7

Table 6a: OLS Results, Decision to Offer Insurance and Percent Eligible

	Decision to Offer					Percent Eligible				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	50,485	0.66	-0.01	-0.00	0.2	47,149	51.6	-2.62	-0.47	1
			(0.02)	(0.02)				(1.67)	(1.76)	
Red-lined industries	8,839	0.6	0.02	0.04	0.4	8,311	42.2	0.24	3.4	0.5
			(0.04)	(0.04)				(3.9)	(4.11)	
Above 75th percentile for % never married males under 35	12,229	0.56	-0.00	0.00	0.0	11,445	39.4	-3.6	-0.36	0.7
			(0.04)	(0.04)				(3.75)	(4.16)	
Above 75th percentile for % young married women with kids	12,479	0.69	-0.00	-0.02	0.2	11,438	52.4	-2.62	2.0	1
			(0.03)	(0.03)				(3.4)	(3.3)	

Table 6b: Weighted Results, Decision to Offer Insurance and Percent Eligible

	Decision to Offer					Percent Eligible				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	50,485	0.53	0.00	0.01	0.1	47,149	42	-2.79	-2.54	1
			(0.02)	(0.02)				(2.11)	(2.41)	
Employee weighted results	50,485	0.85	0.01	-0.01	1.3	47,149	68.1	-0.77	-1.78	0.6
			(0.01)	(0.02)				(1.4)	(1.65)	

Table 7a: OLS Results, Percent of Employees Covered and Take-up

	Percent Employees Covered					Take-up Rate				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	47,598	42.9	**-2.71**	-1.26	1.1	46,641	51.9	-2.78	-1.13	1.0
			(1.48)	(1.54)				(1.71)	(1.78)	
Red-lined industries	8,428	32.1	0.53	0.89	0.3	8,253	43.7	1.47	1.9	0.5
			(3.2)	(3.4)				(4.11)	(4.32)	
Above 75th percentile for % never married males under 35	11,625	31.1	-4.7	-2.3	1.1	11,375	41.5	-5.7	-3.7	1.0
			(3.08)	(3.42)				(3.8)	(4.2)	
Above 75th percentile for % young married women with kids	11,581	41.65	-0.36	-1.67	0.2	11,287	51.9	0.76	-1.66	0.4
			(3.0)	(2.9)				(3.56)	(3.4)	

Table 7b: Weighted Results, Percent of Employees Covered and Take-up

	Percent Employees Covered					Take-up Rate				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	47,598	34.7	**-3.67**	**-4.14**	**3.3**	46,641	41.6	-2.24	-1.19	0.8
			(1.83)	**(2.09)**				(2.15)	(2.45)	
Employee weighted results	47,598	57.7	-0.4	-1.07	0.4	46,641	70	-1.04	-2.48	1
			(1.33)	(1.6)				(1.41)	(1.65)	

Table 8: OLS DD Results, Small Firms and Large Firms Separately

	Small Firms				Large Firms			
	N	Mean	F*Post	P*Post	N	Mean	F*Post	P*Post
Premiums	10,004	189.6	2.6	-3.0	16,647	176.1	**-4.6**	-0.98
			(4.5)	(4.9)			**(2.6)**	(2.6)
Employee Contribution	10,191	28.3	4.3	-1.68	17,882	34.17	-0.68	-1.58
			(2.7)	(2.9)			(1.41)	(1.5)
Decision to Offer	29,003	0.43	**-0.02**	-0.01	21,482	0.96	-0.01	-0.01
			(0.01)	(0.01)			(0.01)	(0.01)
Percent of Workers Covered	28,767	28.5	-1.5	-0.54	18,831	64.8	0.85	0.79
			(0.93)	(0.97)			(0.98)	(1.02)

Table 9: OLS Results for Decision to Offer Insurance, 10 States

	MEPSIC and RWJF			MEPSIC and NEHIS		
	N	Mean	S*Reform*Post	N	Mean	S*Reform*Post
Whole sample	24,437	0.66	-0.03	10,163	0.65	-0.01
			(0.04)			(0.03)

Table 10: Effect of Reform on Underwriting Practices

	N	Mean	F*Post	P*Post
Any prex-or exclusion?	33,094	0.58	**-0.05**	-0.002
			(0.025)	(0.027)

Table 11: Effect of Full Reform vs. All Else

	N	Mean	S*F*Post
Premiums	26,651	181.1	**7.8**
			(4.2)
Employee Contribution	28,052	32	**5.1**
			(2.4)
Decision to offer	50,485	0.66	-0.01
			(0.01)
Coverage rate	47,598	42.9	**-2.12**
			(1.29)
Medical Underwriting	33,094	0.58	**-0.05**
			(0.02)

CHAPTER 4

CONCLUSION

One out of every six Americans does not have health insurance at any point during the year. A recent opinion poll finds that 67 percent of the public believe that the federal government should provide health insurance to all, but to date Congress has been unable to solve the problems resulting in the growing number of Americans without health insurance.[89] During the past decade, state governments and not Congress took the lead in reforming the private health insurance system. Among the employed, the uninsurance problem is overwhelmingly concentrated in the small-group market, and states aimed to encourage health insurance provision among small firms. The most direct means to achieve higher private insurance levels is to mandate that small firms provide health insurance to their workers. But ERISA regulations enacted in 1974 prevented states from considering this option. Instead, law makers hoped to solve the problem indirectly by regulating insurer behavior. Surveys and anecdotal evidence highlighted difficulties that small employers faced in obtaining policies from insurers, and states concluded that curbing medical underwriting practices would improve avenues for employer provision of health insurance. From 1990 to 1996 only state governments took part in small-group reform. Since then, the federal government has become involved, enacting many of the provisions already undertaken by states through the 1996 Health Insurance

[89]The results of a recent opinion poll on national health insurance are provided on the front page of the Wall Street Journal, Friday October 29,1999.

Portability and Accountability Act.

Evidence presented in this dissertation suggests that rules preventing insurers from distinguishing between high and low-risk groups while requiring them to sell to all comers has not improved the situation for the average small employer. I find that instead, reform has unintended consequences. The negative impact of reform on average and low-risk groups is not startling from an economist's point of view. It is in fact quite predictable. My evaluation of small-group reform at the state level suggests that the recent federal attempt to reform private insurance markets may also back fire.

APPENDIX A

DETAILS OF DATA SET CONSTRUCTION

Data gathering constituted a major component of this project. I document the sources and details of data set construction below in three sections;

(1) Individual-level data used in Chapter 2;

(2) Employer-level data used in Chapter 3 and;

(3) State regulation data used in Chapters 2 and 3.

A.1 Individual-level data for Chapter 2

Individual-level data for Chapter 2 are taken from the March Current Population Surveys (CPS) of various years. Using the March CPSs of 1964-1996, I extracted data for 1988-1996. Since 1988 is the first year that the firm-size variable was collected, historical trends in health insurance provision among small and large-firm employees will only go back this far in time. For 1997, I extracted data from March CPS of 1997 that appears on the Bureau of Labor Statistics (BLS)/Census website (www.bls.census.gov). I extracted data covering more years and more variables than I use in the analysis in Chapter 2 to allow for later extensions of this study. For example, the data sets are created so that I can look at the effect of reform on the probability that anyone (not just full-time workers) receive health insurance from the private insurance market or have any health insurance at all. The gathering of regulatory information was a labor intensive project that I document in

greater detail elsewhere (Simon, 1999) and summarize in Appendix A.3.

1997 CPS Data

Using the 'ferret' data extraction tool, I requested an extract from the March 1997 CPS to be downloaded as ASCII text along with a record layout file. The variables selected are a_age, a_hga, a_lineno, a_maritl, a_reorgn, a_race, a_sex, dis_hp, dis_cs, fownu18, gmstcen, hi, dephi, depriv, oth, priv, out, hiout, pout, cov_hi, cov_gh, hiown, hiemp, hi_yn, otyp_4, otyp_5, hea, hityp, prityp, h_idnum, h_mis, h_numper, wsal_val, marsupwt, clwk, wemind, noemp, wkswork, hrswk and wemocg.

1992-1996 CPS Data

The variables extracted are: age class1 dislim emplsz ghincl _educ hrslyr indmjr leftjb marstat occmjr state wgt grdatn _male _race _spneth _wklywg _wkslyr child18 health covhi cvcpyn cvmdcr hvprhi cvmdcd ghcovr hperct hhid3 whohi1 whohi2 whohi3 whohi4 whohi5 recnum mis lineno and year. These variables correspond to the list extracted for 1997.

Selection of Sample, 1992-1997

After extracting the CPS raw data for 1992 through 1997, I standardized the coding and naming of variables for all years to create a stacked data set. To

ensure that this data set had been created properly, I printed out frequencies and means for each year and made sure that descriptive statistics of variables were similar across the years. I then selected the sample for analysis according to the following criteria: i) those who are private-sector workers ii) those who work full time (30 or more hours a week) iii) those who worked at least 14 weeks last year iv) those aged 16-65 years v) those not in Hawaii vi) those not in mid-sized firms with 25-100 workers. Those with invalid values for wages were excluded if _wklywg/hrslyr was less than 2.5 or greater than 500.

My initial sample consisted of about 873,908 observations. After I deleted workers in mid-sized firms, part-time workers, etc., this number became 273,519. After calculating an hourly wage, responses outside the $2.5-$500/hr were considered invalid and deleted (this rid my sample of 6,308 observations). Deleting respondents from Hawaii further diminished the sample. The final data set has 222,166 observations. I normalize the final sampling weight to add up to the number of observations in a given year.

Specific variables included in the analysis

Dependent variable: This analysis considers the impact of reform on the probability of receiving health insurance though a worker's employer. The dependent variable is an indicator that takes on a value of 1 if the respondent received health insurance

from his/her own employer.

Sample weights: I use the March CPS final sample weight in my analysis. Unweighted results were very similar.

Age: The age of the respondent in years and its square are included.

Sex: An indicator variable controls for the sex of the respondent.

Marital status: I create separate indicator variables for whether the respondent is married, divorced, never married, separated or widowed. I also include this variable interacted with gender.

Education: I use the recorded total years of education to categorize the respondent's education into nine categories corresponding to those who have completed 1-6 th grade , 7-8 th grade, 9th grade, 10th grade, 11th grade, 12th grade, some college, college, and college plus. In the analysis, the highest level of education is excluded.

Race: I use the Hispanic ethnicity and race variables to categorize people as white non-Hispanic, black non-Hispanic, Hispanic and other non-Hispanic.

Physical ability: The March CPS asks whether there is anyone in the household with a disability, and whether the respondent ever left a job for health reasons. These are entered as two separate indicator variables.

 # in household: I include a count of the number of people in the household.

Wage: I use weekly earnings from the previous year divided by the hours worked in a normal week the previous year to compute an hourly wage. If this value falls

outside the $2.5 and $500 an hour range, I exclude the observation.

Hours per week: I include a linear term that records the number of hours that the respondent works during a normal week.

*Weeks last yr:*I include a linear term for the number of weeks that the respondent worked last year.

Industry: Information about the industry in which the respondent worked last year is recorded at the one-digit level. The categories are: agriculture, forestry and fishing, mining, construction, durable manufacturing, nondurable manufacturing, public utilities, wholesale trade, retail trade, finance and insurance, business and repair, personal household services, entertainment and recreation, professional and related service. The last category is omitted in regressions.

Occupation: Thirteen different occupations are listed under the heading 'major occupation for longest job held last year', the last of which is excluded.

Firm size: The respondent is asked about the number of people who work at all locations of the firm as a categorical variable. I define a small firm as one with fewer than 25 workers and I exclude respondents from firms with between 25 and 100 workers. For the larger firms, I include indicators for firms with 100-499 workers and with 500-999 workers. The category with 1000+ workers is omitted.

State: I used the individual's FIPS state code of residence to create a vector of state indicators. New Jersey was the excluded state.

Time: Variable are included to indicate the year of the survey. The excluded year is

1992. I also include interaction terms for state by small firm, state by year, and excluded the New Jersey terms in both cases.

*DDD terms:*I created a variable that took on a value of 1 if an individual worked in a small firm in a state that had full reform in place in that year, and 0 otherwise. Similar terms were created to indicate the presence of partial reform and bare bones laws interacted with whether the worker was employer by a small firm. The resulting three indicator variables are the focus of this study.

Estimation

For the final estimates presented in the chapter, analysis was first performed using the probit procedure available in SAS to regress the dependent variable on the explanatory variables listed above. I then retrieved the resulting coefficients and the variance-covariance matrix and used these statistics to evaluate the marginal effects and their standard errors. This was necessary because the evaluation of the probit coefficients at the sample means of the independent variables would not have produced meaningful marginal effects in this instance. In order to calculate the marginal effect of full reform on the probability that a worker in a small firm received health insurance, I first computed the probability of being insured if all small-firm workers were surveyed in 1992, and no state had experienced reform. I set all the interactions involving time to the base year of 1992, then I select only the small-firm workers and set the three level interaction for a particular type of reform

to 1 and then to 0. I compute the change in the probability of being insured, and the average of this difference across all individuals produces the marginal effect. In order to compute the standard error for this statistic, I used a Taylor series expansion of the non-linear estimator using the 'delta-method'. A similar method is used to compute the marginal effect of partial reform and bare bones laws. Further details are available upon request. Results were also estimated using linear probability models.

A.2 Employer-level data for Chapter 3

The data for this chapter are confidential government information and were obtained with some effort. In the spring of 1997 I tried unsuccessfully to gain access to proprietary private-sector surveys of employer-provided health insurance as well as the BLS Employment Cost Index and Employee Benefits Survey. When the public-use Robert Wood Johnson/ RAND 10 State Employer Survey became available through the University of Michigan's ICPSR data archive in the summer of 1997, I considered using this survey for my dissertation. But it was not ideal for identifying the effects of small-group reform given the limited state and time variation.

At this time, a new survey had just been completed containing data on all states in 1993; the National Employer Health Insurance Survey (NEHIS) at the National Center for Health Statistics. After the submission of a research proposal,

the NEHIS staff allowed me access to the data at the end of 1997 as a special employee of the Center for Disease Control and I became the first outside user of this survey. During the next six months I became familiar with many aspects of the survey such as sample design, editing and imputation techniques as well as the survey data themselves. However, because the NEHIS was only a point-in-time survey, I could only compute a difference-in-difference estimate for the impact of reform on small employers using large employers in the same state as a control group. To compute the impact of reform using a DDD approach, I required a second survey collected at some other point in time.

In 1996 the Medical Expenditure Panel Survey Insurance Component (MEPSIC, essentially the second round of the NEHIS survey) had been carried out by the Agency for Health Care Policy and Research (AHCPR) and the Census Bureau, and was to be available only through the Census Bureau for paying users. I applied for and obtained a dissertation grant from the Health Care Financing Agency to pay the Census Bureau lab fees (as well as my stipend for 12 months). I submitted a research proposal to conduct the analysis in Chapter 3, and it was approved in March of 1999. The data editing was completed in July 1999, and I became the first outside researcher to use the MEPSIC.

Because the text explains in detail the restrictions surrounding the use of the MEPSIC and the NEHIS and the unique technique I use to compute regression estimates, I limit the discussion here to the creation of the data sets. Both data sets were created in an identical fashion to match each other for joint analysis, thus they

will be discussed together.

Creation of MEPSIC and NEHIS data sets

-Data sets for studying offers of health insurance, coverage, eligibility and take-up rates

Starting with the main establishment file, I delete observations on self-employed individuals and respondents from Hawaii.[90] Those establishments with firm size greater than 30 and less than or equal to 60 are also deleted. I merged this sample with the four-digit industry codes for this set of observations (which in both cases are obtained from a separate sampling file), and created another three-digit industry code to match the codes used in the 1990 Public Use Micro Sample (PUMS). I needed this variable in order to merge the establishment file with demographic characteristics of each state-by-3-digit-industry cell computed from the PUMS. If an establishment belongs to a state-by-industry cell that has a number greater than 0.17987 as the share of never-married male workers under 35 years of age (as imputed from the PUMS data), it will be classified as "Above 75th percentile for % never married males under 35" in the regression analysis. Similarly, an establishment that belongs to a cell with a number greater than 0.127172 as the share of married women under the age of 41 yrs with children will be classified as "Above 75th percentile for % young married women with kids". The four-digit

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[90]In the NEHIS the self-employed businesses were in a separate data set while in the MEPSIC they were together with the other establishments.

industry code is also used to identify establishments that belong to the list of red-lined industries in the 1991 survey of insurers conducted by Zeller, McLaughlin and Frick (1992).

After the steps taken above, the resulting data sets constitute the final sample that is used in descriptive statistics on employer characteristics. This is the sample used in regressions where the dependent variable is the employer's decision to offer health insurance. When looking at the percent of workers covered, any establishment which offers health insurance but does not report the number of workers at the establishment who are covered by health insurance are deleted. When looking at the percent of workers offered health insurance, those offering health insurance but not reporting a value for the number of workers at the establishment considered eligible for health insurance are deleted. To look at the take-up of health insurance, those establishments offering health insurance who do not report a value for either the number eligible or the number who receive coverage are deleted.

-Data sets for studying premiums and employee contributions

For those offering health insurance, the sample for looking at premiums and employee contributions is constructed as follows. Starting with the file containing information on all plans offered, I delete observations for which reported premiums are missing or imputed, and in the MEPSIC I deleted cases where the premium

appears too low (below \$132/year, based on the minimum considered valid for the NEHIS sample adjusted for inflation) for creating the premiums file.[91] For creating the employee contributions file, I delete observations for which contributions are missing or imputed. The rest of the file construction details are common for employee contributions and premiums. After sorting the plans by unique establishment identification number (created by concatenating the firm id with the plant id) and the number of enrollees of the plan (including imputed values),[92] I pick the plan with the most number of enrollees for each establishment offering insurance. I then delete observations from firms 30<# of employees<=60, as well as respondents from Hawaii. I merge these data with data from the establishment file created for looking at the decision to offer insurance. Because small-group reforms did not affect small firms that self-insured, I delete these plans from my sample.

- Data sets for looking at underwriting practices and the decision to offer an HMO

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[91]In the NEHIS, the plan file also contained information for single service options such as dental coverage only plans. These were deleted from my sample. The premium and employee contribution values were collected separately for self-insured and commercially insured plans in the NEHIS. In the MEPSIC, I use the question that all plans were asked to respond to. While the amounts are reported as monthly figures for the NEHIS, the MEPSIC asks for annual equivalents. The NEHIS numbers were inflated using the BLS CPI to convert them to 1996 numbers.

[92]For the NEHIS, since the plan file is still being edited, I have used the most recent versions of all variables such as the number of enrollees. There were 32 observations in my sample that were recently identified as 'wrap-plans', that is, plans that went along with other plans and were not offered as separate options. Because it was impossible to combine these plans into one in terms of employee contributions, plan characteristics, etc. I deleted these from my sample.

Here I start with the health insurance plan files and I delete observations that have missing values for either whether the plan is allowed to exclude unhealthy individuals or whether the plan can place pre-existing conditions exclusions on employees. I create an indicator at the establishment level for whether any plan has either of these underwriting practices, and I match this information to the establishment level data for the sub-sample of establishments who offer health insurance (of the sample constructed above for looking at whether an establishment offers health insurance). For looking at whether an establishment offers an HMO plan, I create a similar indicator for whether the establishment offers any HMO plan using the plan file and match this to the sample of firms offering health insurance.

Specific variables included in the analysis

Dependent variables: The health insurance premium is taken to be the amount that an employer pays to the insurer to cover one employee for one month. For self-insured firms, this amount includes administrative costs. I analyze the premium for the single plan since plan characteristics are often available for the single plan option only. Employee contributions are defined as the dollar amount that the employee pays per month in order to receive coverage. An establishment that offers health insurance is considered to experience underwriting practices if an insurer is allowed to impose pre-existing conditions exclusions or exclude an employee for the life of any policy offered by that establishment. Similarly, an establishment offering health insurance is considered to be offering an HMO plan if they offer at least one such

plan.

If an employer offers health insurance to any employees, the employer is considered to be offering insurance for purpose of this analysis. The percent of workers who are offered health insurance is calculated as the number of workers eligible for health insurance at the establishment divided by the total number of workers at the establishment. The percent of workers covered by health insurance is the number of workers who are covered by insurance at the establishment divided by the total number of workers at the establishment. The percent of workers who take-up health insurance is defined as the number of workers at the establishment who are covered by health insurance at the establishment divided by the number of workers at the establishment who are offered health insurance.

Firm size: Number of employees and its square, as well as a dummy for <30 workers

Time: Year dummy, which is also interacted with the small firm dummy and with the state indicators.

State: Separate indicators, all except Hawaii. NJ is the excluded category

Industry: These are for: agriculture forestry and fishing, mining oil and gas extraction, construction, manufacturing, transportation communication and utilities, wholesale, retail, finance insurance and real estate, business and entertainment, professional and related services. The last category is excluded. I created industry codes in the MEPSIC from the 4 digit SIC collapsed to the 2 digit SIC so that they would match perfectly to the NEHIS definitions.

Union presence: This variable is set to 1 if there are any union members at the establishment and 0 otherwise. If the number of union members at the establishment is missing, I create a separate indicator variable and set the missing values to 0.

Age of firm: Linear and quadratic terms. Separate indicator for missing data

Wage distribution: The surveys both record the fraction of workers at the establishment who are low-wage, mid-wage and high wage. I include the first and the last, and when these are missing I include missing indicators and set the values to 0.

Type of plan: I record whether the plan is an HMO, traditional or a mixed type. For large firms, I also record whether the plan is self-insured.

Deductible: The dollar value of the deductible for the single plan is included.

Maximum benefit: This is the lifetime maximum benefit that the plan will pay.

*Copay, coinsurance:*These are defined for inpatient and outpatient services separately. When there is both a preferred provided and non-preferred provider value reported, I use the former.

Covered services: There are indicators for: routine mammograms, adult routine physicals, routine pap smears, child immunizations, well-baby care, well-child care, outpatient prescription drugs, routine dental care, orthodontic care, nursing home care, home health care, inpatient mental illness, outpatient mental illness, and alcohol or substance abuse treatment. When any variable has missing values, I include a variable to indicate missing values and fill in missing values with zeros.

DDD terms: The is computed as whether the establishment belongs to a small firm*

whether the state ever has full reform (or partial reform)* whether the reform is effective in that year. There are two such terms, S*F*POST and S*P*POST.

A.3 State regulation data used in Chapters 2 and 3

Data gathering for coding state reforms between 1990 and 1996 started in the spring of 1997. I collected over 20 secondary sources reporting on small-group health insurance reform and recorded the information provided about each state from each report (the list of publications that I compare follow). These were mostly reports complied by non-profit or government entities, and often those describing early reform adoption were out of print and could only be tracked through inter-library loan. Once discrepancies became evident, I began contacting state insurance departments directly to solicit their opinions. I mailed letters to each state's health insurance regulation contact (names obtained from a list maintained by the National Association of Insurance Commissioners) asking about reforms effective in that state. I also listed the information I had gathered so far from secondary sources about their state in these letters. An example of such a letter follows.

After receiving responses from 36 states, I read through state insurance codes for all 50 states at the Library of Congress to study the small-group insurance legislation first-hand. When only the newer versions of legislation could be found in the statutes, I referred to the actual bills to see what the effective regulations had been during the earlier years. This exercise together with the wealth of information provided by states and secondary sources helped me establish the timing and nature

of reform more precisely. In certain instances, ambiguities in state legislative materials made judgement calls necessary pending responses to inquiries sent to particular state contacts. This background research resulted in a database that I intend to augment.

Below, I summarize information about the types of small-group reforms that took effect between 1990 and 1996.

Abbreviations

FULL=Rating reforms, guaranteed issue reforms as well as portability, pre-existing condition exclusion limits and guaranteed renewal reforms.

PARTIAL=No guaranteed issue reforms but has at least rating reforms

NONE=Does not have either guaranteed issue reforms or rating reforms

Table A1: Reforms Effective by 12/31/90

STATE	FULL	PARTIAL	NONE
AK			X
AL			X
AR			X
AZ			X
CA			X
CO			X
CT			X
DC			X
DE			X
FL			X
GA			X
IA			X
ID			X
IL			X
IN			X
KS			X
KY			X
LA			X
MA			X
MD			X
ME		X	
MI			X
MN			X
MO			X
MS			X
MT			X
NC			X
ND			X
NE			X
NH			X
NJ			X
NM			X
NV			X
NY			X
OH			X
OK			X
OR			X
PA			X
RI			X
SC			X
SD			X
TN			X
TX			X
UT			X
VA			X
VT			X
WA			X
WI			X
WV			X
WY			X

Table A2: Reforms Effective by 12/31/91

STATE	FULL	PARTIAL	NONE
AK			X
AL			X
AR			X
AZ			X
CA			X
CO			X
CT	X		
DC			X
DE		X	
FL			X
GA		X	
IA		X	
ID			X
IL			X
IN			X
KS		X	
KY			X
LA		X	
MA	X		
MD			X
ME		X	
MI			X
MN			X
MO			X
MS			X
MT			X
NC	X		
ND		X	
NE		X	
NH			X
NJ			X
NM		X	
NV			X
NY			X
OH			X
OK			X
OR		X	
PA			X
RI			X
SC		X	
SD		X	
TN			X
TX			X
UT			X
VA			X
VT			X
WA			X
WI			X
WV		X	
WY			X

Table A3: Reforms Effective by 12/31/92

STATE	FULL	PARTIAL	NONE
AK			X
AL			X
AR		X	
AZ			X
CA			X
CO			X
CT	X		
DC			X
DE		X	
FL		X	
GA		X	
IA	X		
ID			X
IL			X
IN		X	
KS	X		
KY			X
LA		X	
MA	X		
MD			X
ME		X	
MI			X
MN			X
MO			X
MS			X
MT			X
NC	X		
ND		X	
NE		X	
NH			X
NJ			X
NM		X	
NV			X
NY			X
OH	X		
OK		X	
OR		X	
PA			X
RI	X		
SC		X	
SD		X	
TN			X
TX			X
UT			X
VA			X
VT	X		
WA			X
WI		X	
WV		X	
WY	X		

Table A4: Reforms Effective by 12/31/93

STATE	FULL	PARTIAL	NONE
AK	X		
AL			X
AR		X	
AZ			X
CA	X		
CO			X
CT	X		
DC			X
DE	X		
FL	X		
GA		X	
IA	X		
ID	X		
IL			X
IN		X	
KS	X		
KY			X
LA		X	
MA	X		
MD			X
ME	X		
MI			X
MN	X		
MO		X	
MS			X
MT	X		
NC	X		
ND		X	
NE		X	
NH		X	
NJ			X
NM		X	
NV			X
NY	X		
OH	X		
OK		X	
OR		X	
PA			X
RI	X		
SC		X	
SD		X	
TN	X		
TX			X
UT			X
VA	X		
VT	X		
WA	X		
WI		X	
WV		X	
WY	X		

Table A5: Reforms Effective by 12/31/94

STATE	FULL	PARTIAL	NONE
AK	X		
AL			X
AR		X	
AZ			X
CA	X		
CO		X	
CT	X		
DC			X
DE	X		
FL	X		
GA		X	
IA	X		
ID	X		
IL		X	
IN		X	
KS	X		
KY			X
LA	X		
MA	X		
MD	X		
ME	X		
MI			X
MN	X		
MO	X		
MS			X
MT	X		
NC	X		
ND	X		
NE	X		
NH		X	
NJ	X		
NM		X	
NV			X
NY	X		
OH	X		
OK	X		
OR		X	
PA			X
RI	X		
SC		X	
SD		X	
TN	X		
TX	X		
UT			X
VA	X		
VT	X		
WA	X		
WI		X	
WV		X	
WY	X		

Table A6: Reforms Effective by 12/31/95

STATE	FULL	PARTIAL	NONE
AK	X		
AL			X
AR		X	
AZ			X
CA	X		
CO	X		
CT	X		
DC			X
DE	X		
FL	X		
GA		X	
IA	X		
ID	X		
IL		X	
IN		X	
KS	X		
KY	X		
LA	X		
MA	X		
MD	X		
ME	X		
MI			X
MN	X		
MO	X		
MS		X	
MT	X		
NC	X		
ND	X		
NE	X		
NH	X		
NJ	X		
NM	X		
NV			X
NY	X		
OH	X		
OK	X		
OR		X	
PA			X
RI	X		
SC	X		
SD	X		
TN	X		
TX	X		
UT		X	
VA	X		
VT	X		
WA	X		
WI		X	
WV		X	
WY	X		

Table A7: Reforms Effective by 12/31/96

STATE	FULL	PARTIAL	NONE
AK	X		
AL			X
AR		X	
AZ			X
CA	X		
CO	X		
CT	X		
DC			X
DE	X		
FL	X		
GA		X	
IA	X		
ID	X		
IL		X	
IN		X	
KS	X		
KY	X		
LA	X		
MA	X		
MD	X		
ME	X		
MI			X
MN	X		
MO	X		
MS		X	
MT	X		
NC	X		
ND	X		
NE	X		
NH	X		
NJ	X		
NM	X		
NV			X
NY	X		
OH	X		
OK	X		
OR		X	
PA			X
RI	X		
SC	X		
SD	X		
TN	X		
TX	X		
UT		X	
VA	X		
VT	X		
WA	X		
WI		X	
WV		X	
WY	X		

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Sample letter sent to a state insurance department

Richard Ford
Alabama Department of Insurance
201 Monroe Street, Suite 1700
Montgomery, Alabama 36104

Dear Mr. Ford,

I am a Ph.D. candidate in Economics at the University of Maryland and I am writing my dissertation on the effects of state small-group health insurance reforms. I am using an establishment-level database to ask whether these reforms helped small firms buy health insurance for their workers.

In order to answer questions about the effects of state reforms, I need to be certain of what insurance reforms were in effect at the time my data were collected. Unfortunately, I have found only secondary sources and I wonder if I could ask you a few questions about these regulations related to the sale of health insurance to small groups during this period.

My questions are

1. What kinds of small group reforms, if any, were in effect in Alabama? It would be extremely helpful if you could give me the bill numbers.
2. What group size did they apply to? And how was group size determined (was it the number of all employees, full-time only, or just eligible employees?)
3. Were small firms able to buy reasonably priced health insurance through Blue Cross/Blue Shield in Alabama prior to reform?
4. Did small firms generally face problems in obtaining coverage for employees?

In the secondary source literature, I have found the following: A 1995 publication

called "Fifty State Profiles: Health Care Reform" by Intergovernmental Health Policy Project (October 1995) does not show any small group reforms in Alabama. Another publication, by the GAO, called "State Efforts to Assist Small Businesses (GAO/HRD 92-90) claims that Alabama adopted premium tax waivers in 1971 to help small employers provide health insurance. Was this true, and if so, was it the case in 1993 as well?

I would be extremely grateful if you could answer these questions for me. Once I am done with my research, I would be happy to provide you with a copy of my dissertation which might be useful to policy makers curious about the effects of these laws.

I obtained your name through the NAIC website as the health insurance contact in your state. However, if there is another person in your department who might have the answers to these questions, I would appreciate it if you would be kind enough to forward it to that person.

Thank you very much for your time,

Sincerely,

Kosali Ilayperuma

Department of Economics	Tel : 301 405 3520
University of Maryland	Email: ilayperu@econ.umd.edu
College Park, MD 20742	fax: 301 405 3542

APPENDIX B

AN ATTEMPT TO RECONCILE FINDINGS IN CHAPTER 2 WITH MONHEIT AND SCHONE (1999)

How Have Reforms Affected the Probability that a Worker is Offered Coverage? Evidence from the February CPS of 1995 matched to March CPS of 1995, the February CPS of 1997 matched to March CPS of 1997, the April CPS of 1993 and the May CPS of 1988.

I compile data from several CPS supplements (various February and May supplements) that contain information on whether a worker was offered coverage, and whether the worker accepted the offer. I create a sample to match that used by Monheit and Schone (1999) as close as possible to see whether the difference in our results could arise due to the difference in specification or the difference in the data sets used, or in the question asked.

In the March CPS information is available only about whether coverage was actually obtained through an own employer. In the February 1995 and 1997 Contingent Worker Supplements workers were asked about benefits provided through the employer, including health insurance. But these surveys did not ask about firm size. Another problem in using the March and the February data together is that one (March) asks health insurance coverage through the a job held last year (did you receive health insurance at any job last year?), while the other (February) uses the week before the interview as the reference period. In addition, no unique

person identifiers are assigned thus matching between individuals is not automatic.

Matching People Between February and March Supplements.

I follow a similar procedure for 1995 as for 1997. I extracted the relevant variables from the March and the February supplements for matching. These include household id, line number of the respondent, the age, gender, race and the state of residence. I first tried to match on just household id and line number but the demographic variables from each survey indicated that the match was poor. I contacted Anne Polivka at the BLS who informed me that she has matched the February and March supplements well using household id, line number as well as state of residence. Once state of residence was added at a matching factor, the match-rate was high (over 90%). I deleted individuals in the first and fifth month rotations in the March CPS and the fourth and the eighth rotations of the February CPS since they would not be in both surveys.

Next, I keep those workers for whom the job last year is the same as the current job so that I can assume the same health insurance variables apply. To do this, I look at whether the three digit industry and the job classification for the current and previous years are the same. In both cases, if an individual changed jobs but stayed withing the same (three digit) industry and occupation, I would not record that as a job changed and I may be using the wrong health insurance information for that individual. It seems unlikely that a job change would not involve a change in either given the detailed nature of the classification. This technique has

been used before (Kapur 1998) to identify job changers. This technique also removed those who have not worked in the previous year from the sample.

Before matching, I delete from the sample individuals I will not include in my final sample. These include individuals younger than 20 and older than 64, non wage earners, military personnel (even if they are currently civilian), unemployed persons, non sample respondents, those retired disabled or otherwise not in the labor force, on layoff. In the March survey I keep the private sector workers and government workers, even though in the February health insurance questions, it is not clear whether any of the self employed people are asked the health insurance offer questions, and thus it is likely that no self employed people are included in the analysis.

The February and March Health Insurance Questions

In the February 1995 survey, the Contingent Worker supplements first asks workers whether they have health insurance through any source (HIANY, PES49). The universe for this is all workers who are not retired, disabled, and otherwise not in the labor force, or are unemployed. I delete those who are not in the universe or do not know the answer to this question. Workers who answer yes to this question, and who are do not have an I01COW code of 6,7, or 11 (as far as I can tell, this means those who are not self employed, although code 11 is not defined anywhere: I have asked BLS about this) are then asked whether this health insurance is received through the current employer (PES50). Those who answer yes to this question are

then asked whether the employer pays all, part or none of the premium (PES51). All these people are working for firms were at least one person had to be offered health insurance, and they themselves were eligible for it. For those who said that the insurance they had was not through the current employer (an answer of no to PES50) are then asked how they obtained their health insurance. There are 12 options such as through an employer, through a spouse, purchased on their own etc (PES52A). Then the next question has a universe of 'Entry in PES52A and IOICOW not equal to 6, 7 or 11' which means those who said they have coverage but its not through the employer. The question then asks whether the employer offered health insurance to any of its employees (PES53). Those who answer yes to this question are then asked whether they could have been part of this plan if they wanted to (PES54). If they said yes to this question, they were asked why they did not take part (PES55). They have 8 options. If they said no to PES54 then they are asked why not (PES56). They have 6 options such as whether it was too expensive, whether they don't work long enough etc.

In the February 1997 supplement, the interviewer first asks workers whether they have health insurance through any source (PES49). The universe for this is all workers who are not retired, disabled, and otherwise not in the labor force, or are unemployed. Workers who answer yes to this question, and who are do not have an I01COW code of 6,7, or 11 (as far as I can tell, this means those who are not self employed, although code 11 is not defined anywhere) are then asked whether this

health insurance is received through the current employer (PES50). Those who answer yes to this question are then asked whether if this employer did not offer health insurance they could have been covered by another family member (PES50A). (note: this is the only difference between this and the 1995 questionnaire). If there is an entry in PES50A then they are asked whether the employer pays all, part or none of the premium (PES51)

From there on the questions proceed as in 1995.

It appears that if the worker is uninsured, we have no way of knowing whether the worker's employer offers health insurance to anyone, let alone to them. I create a variable OFER which has a value of 1 if anyone in that worker's firm was offered health insurance and 0 otherwise. The variable OFFER has a value of 1 only if that particular worker was offered coverage. The health insurance questions only applied to those who have coverage through some avenue, and then self employed people are also excluded from further questions beyond having any health insurance or not. HIEMP has a value of 1 if the coverage was obtained through the employer. For 95 and 97 there is a distinction between this and the GHINCL variable (which comes from the March survey), although in April and May the GHINCL is the same as HIEMP.

The CPS of April 1993

The April Employee Benefits Supplement does not need to be matched to the March since it contains all the necessary variables such as firm size. This variable

is recorded in a different way than the March surveys. A_s35 asks about how many employees are working at all locations. a-s33 asks how many people work in that location, and a-s34 asks whether the firm is a multi establishment firm or not. I call a firm 'small' if it is single location and less than 25 at the establishment, or if it is less than 25 in establishment size and in firm size. If it is a large establishment over 100 (whether single or multilocational), or if is a mid sized establishment but its firm size is greater than 100, its a large firm. If establishment size is medium and it is multilocational, its large. If its single locational and its medium and gets deleted. I kept private workers excluding all self-employed, and kept all government workers. I deleted military workers and those not working for pay.

Health Insurance Questions in April 1993

The health insurance questions start with a-s61. It asks whether the employer offered a health insurance plan to any of its employees. The universe is s32b2=1 and insta=4. s32b2 has a universe of a_lfsr=1 or 2 which means that the person is working or has a job but is not at work.. S32b2=1 if there is an entry of P,F,S,L,I or SE in 23E. 23E is the class or worker question, so this S32b2 criteria means that the worker is either private, fed govt, state govt, local govt, self employed or 'I' which does not stand for anything. If a worker answers yes to question s61, then they are asked (s62) whether they are covered by this plan. If they say yes to s62, they are asked whether they have a pre existing condition that is not covered by this plan (s63). If they said that they are not covered by this plan

(note: typo in universe statement), they are asked why (s64). If they said that they were ineligible or denied coverage, they are asked why (s65a). If they said they elected not to be covered, they are asked why (s66a). For both preceding questions, they get multiple options. If there is an entry in s63 or if they elected not to be covered (s64=2) then they ask you more questions such as whether your employer let you chose between health insurance and cash bonus, (s67B), other benefits instead of health insurance. The s68a asks whether you are covered by a non-employer health insurance plan. The universe is the same as the first health insurance question. There are several options to chose from if they are covered. S70 asks whether you could get health insurance from the employer if you were to retire, and whether this would be at the group rate til 65, and whether the employer would pay part of this cost. The health insurance questions end at s72.

The CPS of May 1988

As with the April 1993, there is no need to match this to a March supplement since this contains all the necessary variables. I recoded the variables such as mar, _race and _male to match the other surveys. I kept those between 20 and 64 yrs of age,. I deleted the armed forces and non workers through industry categories. OFER, GHINCL and OFFER are the health insurance variables. I delete all self employed etc through the variable class1: it does not distinguish between unincorporated and incorporated self-employed workers.

The Health Insurance Question in May 1988

The health insurance questions start at position 577 and asks whether the employer offer health insurance plan. The universe is I74=2, which is an interviewer check item taking values 1, 2, and 9. There are 25,109 obs with value 2. Its says 'i38 1 or 2, I39 1 or 2', and its not clear whether we should read 'or' or 'and' in between the two. Item 38 asks whether you are self employed. 1 or 2 is yes or no, and 9 is for NIU. The universe for this is those who are eligible for the supplement and have a confirmed employer. I39 has 29,468 values of 9, about 2800 values of 1 or 2.I think I39 picks up the self employed who also have a wage and salary job.

The next health insurance question is asked if you said that your employer offers health insurance, Item 81 asks whether you are covered by this health plan. If you said that you were not covered by the plan, you are asked why not. There are 6 options to answer to.

Combining the files

After I made the variables consistent across years, I combined all the files into one data set. I create state codes and region codes, delete all mid sized firm employees, and merge the regulation data in. From there I construct the 1st, 2nd and 3rd level interactions for the analysis and run the linear probability models. I also ran descriptive statistics by year and for the whole sample. To combine data in two year combinations as follows: 1988 and 1997, 1988 and 1995, and 1988 and 1993.

Conclusion

Table B1 shows the descriptive statistics, and Table B2 shows the OLS

results. The question being investigated is whether small-group reform changed the probability that a worker in a small firm was offered health insurance. Table B2 indicates that the impact of reform, defined as in Monheit and Schone (1999), produce negative coefficients when using CPS data, regardless of the years considered. Monheit and Schone (1999) find some positive and statistically significant effects on the probability that a worker was offered health insurance while in Chapter 2 I find negative statistically significant effects on whether the worker actually received coverage. The results in this Appendix indicate that using the CPS data regarding offers of coverage also produce negative effects due to reform.

Table B1 : Descriptive Statistics of Selected Variables by Year

Variable	1988	1993	1995	1997
Percent offered coverage	80.4	77.7	84	84.2
Percent in firms that offer to anyone	84.7	82.9	86	86.2
Percent in small-firms	25.7	25	21.8	22.6
Percent covered by own employer	71.4	67.2	71.7	71.2
Percent married	66.7	64.7	65.1	64.1
Percent male	52.8	51.4	51.8	51.5
Percent white	88.7	88.5	85.7	86.8
Age in years	38	39	39	40
Sampe size	18,262	18,183	22,683	20,404

Table B2: OLS Results of Three-Level Interaction Terms

	1988 and 1997	1988 and 1995	1988 and 1993
Offer rates with state fixed effects			
Any reform vs none	-0.019 (0.29)	-0.06 (0.022)	-0.02 (0.017)
GI vs not	-0.018 (0.018)	-0.04 (0.02)	-0.013 (0.02)
GR vs not	-0.019 (0.029)	-0.04 (0.02)	-0.016 (0.02)
Offer rates with Region Fixed Effects			
Any reform vs none	-0.02 (0.03)	-0.05 (0.02)	-0.02 (0.02)
GI vs not	-0.01 (0.010	-0.03 (0.01)	-0.01 (0.01)
GR vs not	-0.02 (0.03)	-0.04 (0.02)	-0.01 (0.01)

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